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CALIBRATION OF THERMOMETER GREEN 5280, BY HANSEN'S METHOD.

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1. DESCRIPTION.—Thermometer Green 5280 is 20 in. long, with bulb 1.5 in. long, and 0.25 in. in diameter; stem, 18.5 in. long and 0.25 in. in diameter, and bore 17.75 in. long, graduated on the glass to 5ths of a degree F., from 17° to 214°, the length of a graduated degree being about 0.07 in. The distance from the middle of the bulb to the 32° mark is 4.1. in. The stem has a pear-shaped reservoir at the end opposite the bulb, capable of containing a column of mercury about 200° long.

2. CALIBRATION.—*Methods of Observation.*—Calibration corrections were desired for points differing by 5° from 32° to 212°. As required by Hansen's method, and as most convenient for Neumann's method, all possible readings at both ends of all lengths of columns differing by 5°, from 5° to 175°, were made at or near all graduation-marks differing by 5° from 32° to 212°. The column lengths were read running the column of mercury from 32° to 212°, and immediately after running in the opposite direction. The mean of the two column-lengths obtained between any two graduations was taken, thus eliminating any slight effect of change of temperature during the time of making the readings. The resulting observation-equations are of equal weight. The bore is assumed to be uniform for the short distances, generally not exceeding 0°.2 between the end of the columns as read and the nearest degree graduation mark. Readings were made with a microsope with thermometer horizontal. No difficulty was experienced in obtaining any desired length of column within 0°.1 to 0°.3 except in case of columns less than 20° or more than 150° to 160° in length, by the following method: A column of mercury 200° to 250° in length was easily obtained by making that amount of mercury run into the stem, a few slight jars being sufficient to start the column moving; a little manipulation then brought the empty space in the bulb to the junction of the bulb and the stem, when a sudden turn of the thermometer upright broke off the column, and almost as sudden an inversion preserved it. When too large a space was left in the bulb to be filled by heating the thermometer to 140° or 150°. a few drops of mercury were allowed to drop off the end of the column in the stem held upright, good care being taken to stop the operation before the column joined the mercury in the bulb. Then,

the thermometer being heated until the the mercury from the bulb began to appear in the stem, the column already in the stem was run down carefully, and partially joined to the mercury in the bulb, leaving a small bubble on one side of the column; the thermometer being allowed to cool slowly until the desired length of column above this bubble (which remains very nearly stationary) was obtained. The column was broken off at the bubble by a slight twitch or jar. If there are objections to heating the thermometer above a certain temperature column lengths above 10° to 20° or 30° longer than the number of degrees of that temperature, depending on the distance of the 32° point from the bulb, can be obtained by jarring off small drops of mercury from a long column into the reservoir at the top of the stem. It requires much more time, care and patience to obtain a column in this way than in the other. Columns more than about 160° in length were so obtained in this case. It is rather difficult to break off short columns, 5° to 15° in length in the manner first described, the weight of mercury in the short column not giving momentum enough to move it away from the rest of the column readily. A little patience is all that is necessary, however. To be able to read the shorter columns at 32°, a column 10° to 50° long, depending on the temperature at the time, must be broken off and put into the reservoir at the upper end of the tube, out of the way.

Hansen's Method. — Hansen's first method of determination of the graduation-errors of a standard of length, as worked out in his paper "Von der Bestimmung der Theilungsfehler eines gradlinigen Maassstabes," is the one here applied to the calibration of thermometers. The other methods there described may be applied in a similar manner. We wish to obtain the corrections to the thermometer readings. But, for the moment, let (0), (1), (2), &c., to (n) represent the corrections to the graduation-marks at the 32°, &c., &c., to 212°, points respectively (being the corrections at 32°, 52°, 72°, &c., 212° degrees for the short example following, and at 32°, 37°, 42°, &c., 212° for the full work on the thermometer Green 5280). Let w represent the volume of a column of mercury used, being a constant for that column, and i the true (corrected) volume of the bore for the corresponding graduated spaces, also a constant; or, in linear measure, let w_0, w_1, w_2, &c., represent the lengths of the column of mercury used (of constant volume) in the several positions along the tube, these lengths being unequal, owing to the *differences in bore* of the tube, and let i_0, i_1, i_2, &c., represent the lengths of the corresponding graduated spaces, directed to make the included volumes of bore (i) equal, (which lengths will also be unequal, owing to the *same differences in bore* of the tube); and let a_0, a_1, a_2, &c., represent the observed differences of length between the column-length and the graduated spaces, taken as positive when the graduated space is the longer. We shall have from each column length read in each position, an observation-equation of the form

$$-(0)+(1)+w_0+a_0=i_0$$
$$-(1)+(2)+w_1+a_1=i_1$$
$$\text{&c., &c.,}$$

in which $w_0-i_0=w_1-i_1=$&c.$=$a constant throughout a set of observation-equations obtained for one column, the differences of the respective w's and i's from a constant length being due to the same causes respectively. But we wish the corrections to *thermometer-readings*, which will be equal in amount but opposite in sign to the corrections to graduation-marks. To transform the above equations into the proper form for obtaining the corrections to thermometer-readings represent the corrections to *thermometer-readings* by (0), (1), (2), &c., to (n), and change the signs of the remaining terms. Now represent the constant ($=i_0-w_0=i_1-w_1=$&c.) by m, and represent the a's (which are now positive when the column-length is the longer) by $[0]_0$, $[1]_0$, &c., $[0]_1$, $[1]_1$, &c., when the quantity in brackets represents the number of the graduation at the lower end of the column, and the subscript represents the number of the column. $[0]_0$, in the shorter example given, is the a corresponding to the reading of the lower end of the 20° column near the 32° mark, $[1]_1$ that corresponding to the reading of the lower end of the 40° column near the 52° mark, &c., $[0]_0$ in the

longer example corresponds to the reading of the 5° column on the 32° mark, $[1]_1$ of the 10° column on the 37° mark, &c.) We shall then have series of observation-equations as follows:

For the first column,

$$1.\ -(0)+(1)+m_0+[0]_0=0$$
$$2.\ -(1)+(2)+m_0+[1]_0=0$$

&c., &c., to

$$n-1.\ -(n-2)+(n-1)+m_0+[n-2]_0=0$$
$$n.\ -(n-1)+\ \ n\ \ +m_0+[n-1]_0=0$$

For the second column,

$$1.\ -(0)+(2)+m_1+[0]_1=0$$
$$2.\ -(1)+(3)+m_1+[1]_1=0$$

&c., &c., to

$$n-2.\ -(n-3)+(n-1)+m_1+[n-3]_1=0$$
$$n-1.\ -(n-2)+\ \ (n)\ \ +m_1+[n-2]_1=0$$

and so on, till

For the $(n-1)$th column,

$$1.\ -(0)+(n-1)+m_{n-2}+[0]_{n-2}=0$$
$$2.\ -(1)+\ \ (n)\ \ +m_{n-2}+[1]_{n-2}=0$$

And for the nth column,

$$1.\ -(0)+\ \ (n)\ \ +m_{n-1}+[0]_{n-1}=0$$

in which n is the number of columns used in the determination. The last equation is of no use in practice, and its $[0]_{n-1}$ is made equal to zero, and retained for symmetry in the following tables.

Forming the normal equations, Hansen obtains by his method of solution the following general formulæ, from which, by substitution of the observed quantities, the corrections to the thermometer-readings result.

(1). Let

$$\{0\}'=[0]_0+[1]_0+[2]_0+\ldots\ldots+[n-2]_0+[n-1]_0$$
$$\{1\}'=[0]_1+[1]_1+[2]_1+\ldots\ldots+[n-2]_1$$

&c., &c., to

$$\{n-2\}'=[0]_{n-2}+[1]_{n-2}$$
$$\{n-1\}'=[0]_{n-1}$$

(2). and

$$\{0\}=-[0]_0-[0]_1-[0]_2\ldots\ldots-[0]_{n-3}-[0]_{n-2}-[0]_{n-1}$$
$$\{1\}=\ [0]_0-[1]_0-[1]_1\ldots\ldots-[1]_{n-4}-[1]_{n-3}-[1]_{n-2}$$
$$\{2\}=\ [0]_1+[1]_0-[2]_0\ldots\ldots-[2]_{n-5}-[2]_{n-4}-[2]_{n-3}$$

&c., &c., to

$$\{n-2\}=[0]_{n-3}+[1]_{n-4}+[2]_{n-5}\ldots\ldots+[n-3]_0-[n-2]_0-[n-2]_1$$
$$\{n-1\}=[0]_{n-2}+[1]_{n-3}+[2]_{n-4}\ldots\ldots+[n-3]_1+[n-2]_0-[n-1]_0$$
$$\{n\}=[0]_{n-1}+[1]_{n-2}+[2]_{n-3}\ldots\ldots+[n-3]_2+[n-2]_1+[n-1]_0$$

Let k be the symbol for the number of any graduation-mark, $p\ (=\frac{1}{2}n)$ for the middle graduation-mark if n is even, and $q\ (=\frac{1}{2}(n-1))$ for the graduation next below the middle if n is odd. Then let $(s,\ k)$ represent the sum of two corrections at points equi-distant from the end-points, and $(d,\ k)$ their difference, k taking its value from the lower graduation, thus:

$$(s,0)=(0)+(n) \qquad (d,0)=(0)-(n)$$
$$(s,1)=(1)+(n-1) \qquad (d,1)=(1)-(n-1)$$

&c., to &c., to

$$(s,p-1)=(p-1)+(p+1) \qquad (d,p-1)=(p-1)-(p+1)$$
$$(s,p)=2(p) \qquad (d,p)=0$$

or to or to

$$(s,q)=(q)+(q+1) \qquad (d,q)=(q)-(q+1)$$

(3). Let $S_k=\{k\}+\{n-k\}$

to $S_p=2\{p\}$

or to $S_q=\{q\}+\{q+1\}$

(4). $D_k=\{k\}-\{n-k\}$

to $D_{p-1}=\{p-1\}-\{p+1\}$

or to $D_q=\{q\}-\{q+1\}$

(5). and

$K_k=(k+1)\{k\}'+(n-k)\{n-k-1\}'$

to

$K_{p-1}=(p)\{p-1\}'+(n-p+1)\{n-p\}'$

or to

$K_{q-1}=(q)\{q-1\}'+(q+2)\{n-q\}'$

and $K_q=2(q+1)\{q\}'$

Then, making the assumption that the calibration-corrections at 32° and 212° are zero, the formulæ for the determination of the corrections to the thermometer-readings are as follows:

(6). $(s,\ k=\frac{S_0-S_k}{n+1}$, the values of k running from 1 to p or q.

(7). $L_0=D_0$

$$L_1=L_0+\frac{2}{n}K_0$$

$$L_2=L_1+\frac{2}{1)}\left\{K_1+D_1-L_1\right\},$$

or, in general,

$$L_k=L_{k-1}+\frac{2}{k(n-k+1)}\left\{K_{k-1}+\Sigma_1^{k-1}D-\Sigma_1^{k-1}L\right\}$$

in which the values of k run from 2 to p or to $q+1$, Σ^{k-1} D=the sum of D_1, D_2, &c., to D_{k-1}, and Σ^{k-1} L = the sum of L_1, L_2, &c., to L_{k-1}.

(8). $$(d,\ k)=\frac{L_k-D_k}{n+1}.$$

(9). $$(k)=\tfrac{1}{2}\{(s,\ k)+(d,\ k)\}$$
$$(n-k)=\tfrac{1}{2}\{(s,\ k)-(d,\ k)\}$$

The probable errors of the corrections are found as follows: The sum of the squares of the residuals=

(10). $$\Omega=(ll)-\frac{1}{n+1}\Sigma\{k\}^2$$
$$-\frac{1}{n+1}\Sigma\Delta^2{}_k+\frac{1}{2(n+1)}\Sigma L^2{}_k$$
$$-\frac{1}{4(n+1)}\Sigma(k+1)(n-k)\Delta k''^2$$

in which (ll) is the sum of the squares of the absolute terms in the observation-equations; the limits of k in the second term are 0 and n; $\Delta^k=\{k\}'-\{n-k-1\}'$, k having the limits 0 and $p-1$ or $q-1$; the limits of k in the fourth term are 0 and $p-1$ or q; and $\Delta'_k=L_k-L_{k+1}$, except that $\Delta'_{p-1}=L_{p-1}$, or $\Delta'_q=L_q$, the limits being 0 and

$$p(p+1)\Delta''_{p-1},\text{ or }2(q+1)^2\Delta''_q.$$

The weights of the respective corrections increase from the ends towards the middle, corrections equi-distant from the middle having equal weights. The expressions for the weights are generally quite complicated, the values lying between

$$P(1)=P(n-1)=\frac{(n+1)n}{2n-1},$$

and

$$P(p)=\frac{2(n+1)}{3},$$

where n is even. The value of P(q) is quite complicated and of different form for different values of n, and slightly less than $\frac{2}{3}(n+1)$. The probable error of a correction is then:

(11). $$r=0.6745\sqrt{\frac{\Omega}{P(l-n)}},$$

in which l is the number of observation-equations, being the sum of the series of natural numbers from 2 to n, and u is the number of unknowns, being the number of corrections determined plus the number of m's (or column-lengths used), or $2(n-1)$.

Formulæ for the values of m can also be readily deduced if desired, but in this work they are not necessary.

Example.—The tables for the application of the method to the whole work on the thermometer Green 5280 being rather large, the following short example is selected from the work, and only the results of the full work are given. In the shorter example, the 20° column being the shortest used, the corrections are obtained for points 20° apart from 32° to 212°. In this case $n=9$ and $q=4$. The quantities in the following numbered tables are obtained by substitution of the proper quantities in the formulæ of the same numbers respectively. The unit in the numbered tables is the hundredth of a degree. The formulæ (1) and (2) are arranged in vertical lines in tables 1 and 2 for convenience in addition. The manner of forming table 2 from table 1 will be readily seen. The readings and resulting column-lengths are as follows:

TABLE A.

1st reading.		2d reading.		Resulting mean column-length.
°	°	°	°	°
32.01	52.08	31.98	52.04	20.06
52.03	72.18	51.84	72.00	.16
72.00	92.06	72.03	92.08	.06
92.04	112.18	91.90	112.00	.12
112.16	132.20	112.10	132.18	.06
132.12	152.22	131.80	151.87	.08
152.00	172.14	151.85	172.01	.15
172.00	192.04	172.18	192.22	.04
191.96	211.98	191.98	211.99	.02
31.99	72.21	31.68	71.98	40.28*
51.95	92.15	51.82	92.04	.21
71.98	112.18	71.82	112.03	.20
92.00	132.20	91.81	132.00	.20
112 07	152.23	111.83	152.02	.18
131.93	172.16	131.78	172.02	.24
151.92	192.16	151.88	192 16	.26
171.96	212.04	171.98	212.08	.09
32.00	91.98	32.00	91.99	59.98
52.08	112.12	51.94	111.97	60.04
72.05	132.00	72.04	131.99	59.95
91 94	151 86	92.19	152.18	.96
111.98	171.88	112.04	171.99	.93
131.99	191.96	132.14	192 10	.96
152.07	212.00	152.00	211.94	.94
32.03	112.25	31.76	112 00	80.23
51.94	132.12	51.83	132.08	.21
72.03	152.18	71.90	152.03	.14
91 96	172.17	91.85	172.10	.23
112.00	192.18	111.81	191.98	.18
132.00	212.15	131.83	212.00	.16
31.94	132.17	31.80	132.00	100.22
51.94	152 12	51.83	152.06	.20
72.14	172.34	71.85	172.08	.22
91.96	192.16	91.81	192.01	.21
111.97	212.03	111 97	212 02	.06
31.98	151.80	32.19	152.00	119.82
51.99	171.82	52.18	172.02	.84
71.98	191.76	72.16	191.96	.79
92.00	211.77	92.22	212.00	.78
32.06	171.97	32.11	171.98	139.89
51.98	191.79	52.03	191.83	.80
71.98	211.64	72.30	212.01	.68
32.18	192.22	31.97	192.00	160.04
51.98	211.94	52.08	212.02	159.95

* 3d reading, 32°.03, 72°.34.

TABLE 1.

(0)′	(1)′	(2)′	(3)′	(4)′	(5)′	(6)′	(7)′	(8)′
+06	+28	−02	+23	+22	−18	−11	+04	00
+16	+21	+04	+21	+20	−16	−20	−05	
+06	+20	−05	+14	+22	−21	−32		
+12	+20	−04	+23	+21	−22			
+06	+18	−07	+18	+06				
+08	+24	−04	+16					
+15	+26	−06						
+04	+09							
+02								
+75	+166	−24	+115	+91	−77	−63	−01	00
1	2	3	4	5	6	7	8	9
+75	+332	−72	+460	+455	+462	−441	−08	00

TABLE 2.

(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
−06	+06	+28	−02	+23	+22	−18	−11	+04	00
−28	−16	+16	+21	+04	+21	+20	−16	−20	−05
+02	−21	−06	+06	+20	−05	+14	+22	−21	−32
−23	−04	−20	−12	+12	+20	−04	+23	+21	−22
−22	−21	+05	−20	−06	+06	+18	−07	+18	+06
+18	−20	−14	+04	−18	−08	+08	+24	−04	+16
+11	+16	−22	−23	+07	−24	−15	+15	+26	−06
−04	+20	+21	−21	−18	+04	−26	−04	+04	+09
00	+05	+32	+22	−06	−16	+06	−09	−02	+02
−52	−35	+40	−25	+18	+20	+03	+37	+26	−32

Check on computation of $\{k\}$:
$\Sigma\{k\}=0.$

TABLE 3.

S_0	=	− 52	− 32	= − 84
S_1	=	− 35	+ 26	= − 09
S_2	=	+ 40	+ 37	= + 77
S_3	=	− 25	+ 03	= − 22
S_4	=	+ 18	+ 20	= + 38

Check: $\Sigma S \begin{cases} =0 \text{ if } n \text{ is odd.} \\ =\{p\} \text{ if } n \text{ is even.} \end{cases}$

TABLE 4.

		Multiplier.	Product.
D_0	$=-52+32=-20$	n or 9	−180
D_1	$=-35-26=-61$	$n-2$ or 7	−427
D_2	$=+40-37=+03$	$n-4$ or 5	+ 15
D_3	$=-25-03=-28$	$n-6$ or 3	− 84
D_4	$=+18-20=-02$	$n-8$ or 1	− 2
		$-\frac{1}{2}$ sum	$=+339$

TABLE 5.

K_0	=	+ 75	+ 00	= + 75
K_1	=	+332	− 08	= +324
K_2	=	− 72	−441	= −513
K_3	=	+460	−462	= − 02
K_4	=	2	×455	= +910

Check on computation of $\{k\}'$, D, and K: $-\frac{1}{2}$ the sum of the multiplied D's (last column of table 4)

$\begin{cases} =\Sigma K-\frac{1}{2}Kq \text{ if } n \text{ is odd.} \\ =\Sigma K \text{ if } n \text{ is even.} \end{cases}$

TABLE 6.

$$(s, 1) = \tfrac{1}{10}(-84+9) = -07.5$$
$$(s, 2) = \tfrac{1}{10}(-84-77) = -16.1$$
$$(s, 3) = \tfrac{1}{10}(-84+22) = -06.2$$
$$(s, 4) = \tfrac{1}{10}(-84-38) = -12.2$$

Check: $\Sigma(s, k)$

$$\begin{cases} = \dfrac{1}{n+1}(q+1)S_0 \text{ if } n \text{ is odd.} \\ = \dfrac{1}{n+1}\left((p+1)S_0 - \{p\}\right) \text{ if } n \text{ is even.} \end{cases}$$

TABLE 7.

$$L_0 = D_0 = -20$$
$$L_1 = -20 + \frac{2}{9}(75) = -3.33$$
$$L_2 = -3.33 + \frac{2}{2\times 8}(+324-61+3.33) = +29.96$$
$$L_3 = +29.96 + \frac{2}{3\times 7}(-513-58-26.63) = -26.96$$
$$L_4 = -26.96 + \frac{2}{4\times 6}(-2-86+00.33) = -34.27$$
$$L_5 = -34.27 + \frac{2}{5\times 5}(+910-88+34.60) = +34.26$$

Check: $\begin{cases} L_q = -L_{q+1} \text{ if } n \text{ is odd.} \\ L_p = 0 \text{ if } n \text{ is even.} \end{cases}$

TABLE 8.

$$(d, 1) = \tfrac{1}{10}(-3.3+61) = +05.77$$
$$(d, 2) = \tfrac{1}{10}(+30.0-03) = +02.70$$
$$(d, 3) = \tfrac{1}{10}(-27.0+28) = +00.10$$
$$(d, 4) = \tfrac{1}{10}(-34.3+02) = -03.23$$

Check: $\dfrac{1}{n+1}\left(\underset{\Sigma_1}{p \text{ or } q}\ \underset{L-\Sigma_1}{p \text{ or } q}\ \underset{D}{}\right) = \Sigma(d, k).$

TABLE 9.

$$(0) = (32) = 00$$
$$(1) = (52) = \tfrac{1}{2}(-07.5+05.8) = -01$$
$$(2) = (72) = \tfrac{1}{2}(-16.1+02.7) = -07$$
$$(3) = (92) = \tfrac{1}{2}(-06.2+00.1) = -03$$
$$(4) = (112) = \tfrac{1}{2}(-12.2-03.2) = -08$$
$$(5) = (132) = \tfrac{1}{2}(-12.2+03.2) = -04$$
$$(6) = (152) = \tfrac{1}{2}(-06.2-00.1) = -03$$
$$(7) = (172) = \tfrac{1}{2}(-16.1-02.7) = -09$$
$$(8) = (192) = \tfrac{1}{2}(-07.5-05.8) = -07$$
$$(9) = (212) = 00$$

Check: $\Sigma(k)$ $\begin{cases} = \Sigma(s, k) \text{ if } n \text{ is odd.} \\ = \Sigma(s, k) - (p) \text{ if } n \text{ is even.} \end{cases}$

TABLE 10.

	Squares.
$\Delta_0 = +75+00 = +75$	5625
$\Delta_1 = +166+01 = +167$	27889
$\Delta_2 = -24+63 = +39$	1521
$\Delta_3 = +115+77 = +192$	36864
	71899

		Squares.	Multipliers.	Products.
$\Delta'_0 = -20+3 = -17$		289	1×9	2601
$\Delta'_1 = -3-30 = -33$		1089	2×8	17424
$\Delta'_2 = +30+27 = +57$		3249	3×7	68229
$\Delta'_3 = -27+34 = +7$		49	4×6	1176
$\Delta'_4 = -34$	$= -34$	1156	2×5²	57800
				147230

$$(ll) = +11872$$
$$-\tfrac{1}{10}\Sigma\{k\}^2 = -996$$
$$-\tfrac{1}{10}\Sigma\Delta^2 k = -7190$$
$$+\tfrac{1}{240}\Sigma L^2 k = +160$$
$$-\tfrac{1}{40}\Sigma(k+1)(n-k)\Delta''^2 k = -3681$$
$$\Omega = +265$$

TABLE 11.

$$P(1) = P(8) = \frac{10\times 9}{18-1} = 5.29$$

$$P(4) = P(5) = \frac{1120}{171} = 6.55$$

$$r(1) = r(8) = 0.6745\sqrt{\frac{265}{5.29(44-16)}} = \pm 0^\circ.009$$

$$r(4) = r(5) = 0.6745\sqrt{\frac{265}{6.55(44-16)}} = \pm 0^\circ.007$$

The calibration-corrections as obtained by Hansen's method for every 5° point from 32° to 212° are given in the first column of table B following. n in this case is 36 and p 18. The calibration-corrections were also obtained by Neumann's method, (see *Am. Jour. Sci.*, Vol. XXI., May, 1881, Art. XLVII., for method,) and the same values were obtained throughout except for the 82°, 87°, 92°, 117°, 122°, 127°, points, when to the nearst hundredth they are 0°.01 larger than those obtained by Hansen's

method. The probable errors of corrections obtained by Hansen's method, carried to thousandths of a degree, range from 0°.0026 at 122° to 0°.0030 at 37° and 207°; and by Thiessen's method of obtaining the probable errors of corrections obtained by Neumann's method, they are 0°.003.

3. OTHER CORRECTIONS. — The boiling point was determined three times within two hours, alternating with determinations of the freezing point. The readings with the thermometer horizontal were in order, July 12, 1882, 6 P. M., 211°.00, 31°.38, 210°.80, 31°.30, 210°.78, 31°.30, 7:40 P. M. The readings at boiling point with thermometer vertical were, in the same order, 210°.60, 210°.50, 210°.58. The barometer-reading at 6:36 P. M. was 29in. .195, with corrections of −0.126 for temperature, and +0.017 for instrumental error, to be applied. The thermometer was 46.80 feet below the barometer, and was approximately 600 feet above sea-level Taking the mean of the boiling - points observed (horizontal), 210°.86, and the boiling-point for observed barometer, with the proper corrections applied, 210°.67, the correction −0°.19 to thermometer-readings in vicinity of 212° results. The correction to thermometer-readings near 32° is, from the last reading above, +0°.70. The length of column between freezing and boiling points is then 180°.89, and the "scale-corrections," or corrections to thermometer-readings on account of this excess of length over the graduated length, result as given in table B, second column.

The constant corrections to the thermometer-readings, given in the third column are the sums of the corrections in columns 1 and 2. Application of the freezing-point correction at any date to these corrections will give the total corrections to the thermometer-readings on that date.

The thermometer was compared on January 1, 1882, with thermometer Casella 21472, (now in the office of the Chief Signal Officer, U. S. A.), whose corrections to an air-thermometer have been well determined, and the corrections to Green 5280 to reduce to an air thermometer (with the freezing-point correction for that date subtracted), resulted as given in the fourth column.

TABLE B.—CORRECTIONS TO THERMOMETER GREEN 5280.

No. of graduation.	Calibration corrections.	Scale corrections.	Total *constant* correction.	Constant correction to reduce to air-thermometer from Casella 21472.
32°	0°.00	0°.00	0°.00	
37	— .02	— .02	— .04	
42	— .04	— .05	— .09	— 0°.17
47	— .02	— .07	— .09	
52	— .01	— .10	— .11	— 0°.21
57	— .07	— .12	— .19	
62	— .04	— .15	— 19	— 0°.36
67	— .05	— .17	— .22	
72	— .07	— .20	— .27	— 0°.39
77	— .05	— .22	— .27	
82	— .05	— .25	— .30	— 0°.40
87	— .05	— .27	— .32	
92	— .03	— .30	— .33	— 0°.45
97	— .00	— .32	— .32	
102	— .02	— .35	— .37	
107	— .04	— .37	— .41	
112	— .08	— .40	— .48	
117	— .06	— .42	— .48	
122	— .04	— .44	— .48	
127	— .04	— .47	— .51	
132	— .04	— .50	— .54	
137	— .04	— .52	— .56	
142	— .04	— .54	— .58	
147	— .01	— .57	— .58	
152	— .03	— .59	— .62	
157	— .09	— .62	— .71	
162	— .07	— .64	— .71	
167	— .06	— .67	— .73	
172	— .09	— .69	— .78	
177	— .08	— .72	— .80	
182	— .05	— .74	— .79	
187	— .06	— .77	— .83	
192	— .07	— .79	— .86	
197	— .03	— .82	— .85	
202	— .04	— .84	— .88	
207	— .02	— .87	— .89	
212	— .00	— .89	— .89	

NATURAL oil, though possessing several advantages as a lubricant over vegetable or mineral oils, has the disadvantage of wanting the necessary viscosity and solidity to resist great pressures. M. Boulfroy has remedied this defect by concentrating natural oil in a special apparatus until it acquires a density of from 0.91 to 0.912. On continuing the process of concentration, a mineral tallow is obtained, which takes the place of animal tallow.

CANAL PROJECTS OF THE DAY.

By F. N. NEWCOME.

From the "Nautical Magazine."

Two engineering projects of surpassing interest have riveted the world's attention during the past few sessions. Through the pre-eminent attention paid to these, and the varied feelings they have called into action both at home and abroad, other propositions of almost equal audacity and merit have been purely eclipsed. Projects that a generation ago would have literally astounded the world, have scarcely evoked a moment's controversy, barely even a partisan championship; while volumes, rather let us say libraries full, have been written about the Panama Canal and the Channel Tunnel. Yet many of the schemes so ignominiously dismissed in a few short sentences are more realizable, more useful, and more certain to bear fruit. When completed, as many of these less imposing enterprises will be in time—and this is more than can be confidently predicted of the Channel Tunnel—they will exercise an influence over politics and commerce, not much inferior to that exerted by the Suez waterway. Unquestionably, however, the Panama venture must be allowed to take first rank among all marine enterprises of the day. It stands *facile princeps*, and must remain so, perhaps, till the end of time. Besides, its consummation appears to be within measurable distance. The capital required to construct it is fully subscribed; serious operations have been commenced; an army of excavators are toiling day and night, while M. de Lesseps, whose name as regards canals is one to conjure with, has distinctly promised its fulfilment by the year "eighteen-eighty-eight." Should his prophecy come true then, or even should his splendid undertaking be achieved at some later date, the mercantile world will undergo a revolution, besides which, all previous transitions will fade and grow dim. England, at any rate, will lose her natural Oriental supremacy—the favored merchants of the Eastern American States competing with other on more than even terms in the rich markets of Japan, China, and the Indies. This unequal emulation will doubtless lead to another serious diminution in the profits upon our trade with Oriental nations, if not to a positive falling away in its volume, while, as regards America, we must expect to see her Mercantile Marine assuming an importance to which it has been a stranger since the iron age dawned across the shipbuilding horizon. What that mutation destroyed, the Panama Canal may re-create. Given a seaway between the Atlantic and Pacific Oceans, almost within her own territories, and the youthful Titan is certain to set her feet astride the ocean. Destiny is evidently "shaping the ends" of the giantess Republic, decreeing for her offspring a first place among maritime people.

But that ocean passage is not likely to be an unmixed blessing to the United States. Its imminence is already raising jealousy and disquietude among the nations; it is likewise drawing attention to the superior advantages possessed by Georgia, Florida, and other States on the Mexican Gulf, with regard to position, harbors, resources, and distance from the Eastern markets. If the South should become a great center of the ship-building industries, and manufacturing capital be transferred thereto from the over-crowded North-East, it may possibly involve a revival of the ancient feud between the North and South. The West is already separated from New York and Washington in all save nominal allegiance. Between the protected manufacturer and the Western farmer there is little in common; and now that free-trade lessons have educated the latter into regarding his rival as a creature who fattens by the sweat of another's brow, the last strong link of sympathy is snapped. There is magic in a name, and it is simply the magic of the word "greatness" which now keeps the East and West in cohesion. It was not yesterday that the latter began paying attention to its own ports along the Pacific, and to its own chances of manufacturing

successfully, and with each diversion of traffic from the San Francisco to the Colon-Panama railway route the agitation recommences. When the seaway is open commerce will be entirely turned aside from the trans-continental route. It will travel by ocean direct from New York, Boston, Brunswick, or New Orleans to its destination, Californians deriving no particle of advantage from its transport. What may we expect them to do in self-defence? To sit still and be half ruined, or to buckle to and manufacture and export for themselves? Is it not reason to suppose that they will try to turn their coal, iron, and copper to account, dispensing with New York as far as may be? New Orleans, again, instead of sending the raw product North as now, is certain to reel and weave its own cotton when once the China ports are brought within a few days steam. Economic laws may be over-ridden for a time, circumstances, indeed, may justify such a course, but when the producing country suddenly finds itself placed in juxtaposition with its customers, it is not likely to allow far-off States to divide its profits. As things now stand it may not pay New Orleans to manufacture and ship its own cotton, but with the Panama Canal open it will, and should the change we anticipate take place, Eastern interests will seriously suffer.

Further, a considerable portion of the European grain supply comes from ports on the Pacific or Gulf of Mexico; Californian growers finding it pay them better to ship around Cape Horn to London than to send across continent to the Atlantic ports. When, however, vessels can steam through Panama, from San Francisco or Diego to Europe, the greater portion of the Western grain trade (not only from districts contiguous to the coast, but from places far inland) will be diverted, ceasing to pass over the Northern railway lines.

However, there is another aspect to this interesting problem. As remarked in a former article on "Shipping Prospects in China," there is a possibility that the trade of this country may benefit from M. de Lesseps' enterprise, at least for a time, perhaps even in the long run. Divided by such enormous distances from the Oriental markets as the Eastern States now are, a transfer of capital and energy to the Pacific must have resulted sooner or later, and it may be that by connecting the Atlantic and Pacific this transition may be retarded rather than hastened. Instead of fixed capital transferring itself from the East to the West, it is probable that the manufacturers of New York and Pennsylvania, aided by the shortened passage and other facilities afforded by the canal, will endeavor to hold their own against the competition arising on the Pacific and Mexican Gulf.

Another canal, uniting the Caribbean Sea with the Pacific Ocean, is projected. Having been taken under the patronage of the American people, and being strongly supported by the Washington Senate, it is quite likely to be open for traffic before the first vessel steams merrily through the great French undertaking. Although not a sod has yet been turned, and although the gross distance from inlet to exit will be considerably longer—about 180 miles against less than a quarter that distance dividing Limon Bay and Paraiso, the point where M. de Lesseps' canal enters the Pacific—the physical difficulties are by United States engineers esteemed inferior to those presented by the rival scheme. It may therefore prove, after all, the pioneer enterprise. It follows the natural line mapped out by the river St. Juan and Lake Nicaragua. Should it at any time be finished, it will doubtless become the more fashionable route, lying some parallels further from the equator. The one great disadvantage it offers is the high summit level—1,076 feet, which must be attained by lockage. On the other hand, the Panama project dispenses entirely with all such contrivances.

Ead's Tehuantepec ship-railway is also meeting with keen support, and like its rivals, will probably become *un fait accompli*, before this century closes. As a first result of its anticipated success, a vigorous effort is being made to excavate a passage across the Florida peninsula. Although the two schemes have no distinct reference to each other, the Tehuantepec route will vastly benefit by the construction of the Florida channel. The other projects to which we had been alluding are beside its influence; traffic from New York to Port St. Juan or

Colon would gain nothing in distance by following the proposed canal route, and might lose considerably in time and money, whereas steamers bound for Campeachy Bay, or other places on the Mexican Gulf, would find it economical to avoid the long sea voyage around Cape Florida. But the towns which would most benefit from the Florida enterprise are Galveston, and those at the mouth of the Mississippi. These will be brought several days nearer to the Northern States, while the passage from New Orleans to Europe will be shortened some hundreds of miles. Sir E. J. Reed proposed an alternative scheme, namely, to construct a ship railway across Florida, and it is quite likely that in the course of time his proposition may be revived, especially should Captain Ead's railway prove as successful as hoped. To commerce between New York and the East, the advantages of the Florida-Tehuantepec route are manifest as compared with the Panama line; the saving in distance to Honolulu and the Japanese and Chinese ports will be enormous — at the very least a thousand miles; while, as regards local trade between ports on the Gulf of Mexico and those along the Northern Pacific, all attempts at comparison are out of the question. A glance at a large map of the world will reveal the immeasurable superiority of his route; indeed, supposing it to be successfully completed, it is difficult to perceive what commerce could select the Panama and Nicaragua routes. European trade with the Orient is sure to prefer the Suez Canal, so will a portion of that issuing from the North Atlantic ports of America, while such percentage of the same commerce as selects the inter-oceanic route will certainly let its choice rest upon Tehuantepec. As a fact, then, supposing Mr. Ead's project should be accomplished, the Panama waterway could only command the shipping passing from North Atlantic ports in Europe and the United States to ports on the West coast of South America, and even this, its legitimate right might be seriously compromised by the questions of climate and expense. When near the equator, ten degrees is a consideration, besides which Ead's railway is contemplated to cost little more than one-third of the estimated expenditure on the Panama Canal. Admitting there is a large under-computation somewhere, the balance still seems to incline upon one side, and should experience demonstrate the feasibility of these ship-railways, shareholders in rival concerns will have little reason to plume themselves upon their good fortune. On the other hand, a canal once dug costs comparatively little to keep in order or work, whereas haulage, power and maintenance of the proposed railway will prove a terrible tax upon the annual incoming. Actual questions, however, over an extended period will alone solve these moot problems.

Another scheme demanding notice is the proposed junction between the Bay of Biscay and the Gulf of Lyons. Such a communication may be said to exist at the present day. The waters of the Garonne, passing Bordeaux, flow through Marmande and Agen to Toulouse, where they unite with those of the Canal du Midi, or, as it was formerly called, the Languedoc Canal. According to an eminent French engineer, M. Abt, the new waterway is to begin at Bordeaux and end at Narbonne, running parallel with the Garonne as far as Castels, at which point it will join the Canal Latéral as far as Mas d'Ageny. From thence it will diverge from the present canal and pass through Agen, Gers, and St. Genès to Montech, where it will again unite with the Canal Latéral. Subsequently at Toulouse, it will connect itself with the Canal du Midi, the united waters flowing together into the Valley of Bastide d'Anjou, after which it will pursue an independent course to the Mediterranean, passing Carcassonne, Lézignan, and Béziers. Practically, then, the French Chauvinists purpose utilizing in part an existing channel, deepening the streams sufficiently to permit of the largest men-of-war passing through. It is very doubtful whether this undertaking could be made to pay commercially. The distance to be travelled is immense, while the various gradients met with along its route will necessitate a multitude of locks. Besides, its original cost is estimated by the State engineers at 1,500,000,000 francs, or £60,000,000 sterling. Supposing even it could be constructed for half that sum, which is the lowest estimate that has been formed, a dividend of only four per cent. would require £1,200,-

000 per annum. How is it to be earned? The task seems hopeless. If an excessive tariff is charged, merchant vessels will prefer to pass through the Straits of Gibraltar, or if patronage is courted by moderation, dividend earning will be out of the question. As a purely business speculation, then, this project seems doomed to failure; nevertheless, it would pay the French nation well to construct. Politically, it would render them independent of that *bête noir* "Gib," at the same time developing the commerce of the whole empire. Before long a vast population will line its banks; large towns will arise in every convenient locality; a hundred industries, now undreamt of, will spring into existence, while the country for leagues on each side will be opened up. Havre, Brest, and the other English Channel and Biscayan ports—indeed all Northern France will benefit as well. Commerce with the Mediterranean and the East will be revivified and developed, besides which many new home markets will be opened up. Should the canal lead to the resurgence of French cotton manufacturing industries, as seems inevitable, and to nothing else, it would be cheap to France at treble any sum it is likely to cost. What a splendid gift from the French nation to itself this seaway would be! A small present sacrifice, a mere passing of wealth from the hands of the thousands into those of the hundreds, to be diffused again throughout France in a few months, perhaps days, and the Gallic race would in one moment attain a degree of influence, importance and prosperity which fifty centuries could not endow it with while its northern and southern ports are so widely separated. Before this splendid ambition our own projects for making Manchester a seaport and connecting the waters of the Bristol and English Channel sink into insignificance. From a commercial aspect both these enterprises deserve the name of "great," but they have little or no political bearing; scarcely more so than the more modest proposition for severing the narrow neck of land that now divides the sister Lochs Tarbet. Granting that ocean liners will steam uninterruptedly up the inky Irwell to Manchester, the question is purely one for Lancashire to consider, and even supposing that good-sized ships traverse the isthmus which divides Bridgewater Bay from the estuary of the Exe, the consequences and the gains will be ours alone to count. Far different with the French aspiration, which, if ever consummated, must revolutionize the political and mercantile *status* of Europe, and render France, in point of strategical position, the most powerful factor in Western politics. Gibralter, as a military station, would lose half its significance. One European nation—the greatest—will be rendered independent of the "Pillars of Hercules," holding in its own right the chief ocean-way between the Atlantic and *Mare Internum*.

England, with uncalculable interests to defend in the East, may well pray for the non-achievement of this speculative ambition, for it would necessitate her doubling her Mediterranean fleet, to avoid a sudden concentration of the Cherbourg, Brest and Toulon ships. It has been reckoned that the men-of-war harbored at Brest could reach Port Said, *viâ* the Garonne Canal, some four days earlier than the English Channel cruisers, who would have to round the "southern rock." For our influence, then, it may be hoped that the labors of construction may prove too severe for the French engineers. Still, our greatness should render us free from petty jealousy at the rising splendor of another power. Where, however, we should feel the shoe pinch most, would be in our trade with the East. At present some portion of the silks and other Oriental wares consumed in the north of France, are shipped direct to England, and thence re-shipped to ports on the French coast; with the ship-canal in existence this commerce would cease. We should also lose our profits on the £5,000,000 worth of French products now sent to our shores for transhipment, or, at least, upon a large proportion thereof. In many other ways we should feel its adverse effects. Our shipping industries would receive a violent shock, while those of France would spring into fresh activity; our flag would be less frequently seen in eastern waters, while theirs would cease to be a stranger, and if the French people desired to avenge Waterloo, they could do so by freeing the canal to native-owned ships, and charging high dues to those bearing a foreign emblem.

Having entered upon a new course of enterprise at home and abroad, which even the death of Gambetta will not terminate, it is improbable that French Chauvinism will long be baulked in this favorite ambition; besides, canal digging has become an actual mania with our excitable neighbors. Millions upon millions of francs are being spent by the Government on public works. The Seine is being deepened between Paris and Rouen, and a magnificent project for making Paris a seaport is slowly fructifying. M. Boquet de la Gaye, hydrographer to the navy, proposes to dredge the river deep enough to allow vessels drawing twenty-three feet of water to ascend to the capital. Although the difficulties are stupendous, and the cost will be enormous, and although engineers and capitalists look grave, the gay city has not lost heart, and at some time in history will essay to place herself on an equality with London. Great attempts are also being made to improve the Rhone navigation between Lyons and the sea, and before many decades are past, China steamers may be seen alongside the wharfs of the provincial capital.

In foreign parts the French canalizing spirit is equally alert. M. Deloncle and M. de Lesseps have between them decreed the separation of the Malay peninsula. The isthmus of Kraw is to be cut through at its narrowest point, shortening the voyage for steamers some three days or more. This will be another blow to England's commercial supremacy. If Euro-Oriental traffic is diverted from the Straits of Malacca, the star of Singapore will set. Both this town and Malacca owe their fame and sudden rise to the trade between the Orient and Occident passing through these straits. Singapore affords a convenient place of call, forms an admirable coaling station; but given a change in the route taken by the China ships, and this resplendent settlement will lose its *raison d'être;* its vast population, existing mostly by their commerce with passing vessels, will migrate in shoals to places where the battle of life may be fought on fairer terms. Trade between Singapore and vicinal ports could not maintain one quarter the present inhabitants, and in a lesser degree the same argument applies to Malacca. Saigon—the French settlement—it is supposed, will benefit and grow rich by the ruin of its British rival. The Kraw Canal project and French activity in the Indo-Chinese peninsula are engendered by the same restive spirit, the same craving after colonial empire, the same yearning after glory. Nothing will subdue or bridle this unfortunate longing for renown except repeated disappointments and the constant eating of Dead Sea fruit.

On the face, it does seem probable that Saigon would arise on the ashes of Singapore, but the vagaries of trade are so unaccountable that it would not be surprising to see the French colony neglected, and new stations springing up at the entrances to the sea-way. This would only be re-acting the experience of Suez, Ismailia and Port Said. French designs might also be defeated through the construction of a second canal or ship railway by English capital, in which case M. Deloncle's calculations as to profits and receipts might require scaling some eighty per cent. Several sections of the peninsula are devisable, and if one canal is made to pay it will certainly provoke emulation before long. These are considerations which investors in these high-flown speculations will do well to take to heart; nevertheless, as political and commercial problems, they demand the earnest attention of our Government.

In Northern Africa, Commandant Rouvaire's proposition for flooding the Tunis-Algerian chotts and thereby re-establishing the Lake Tritones known to the ancients, is coming within measurable distance of experiment. If the "chotts" between Gabes and Biskra are flooded, as M. de Lesseps says they will be ere long, an inland sea, in size nearly fifteen times that of the Lake of Geneva, will have been created by the work of man's hands. The prospect is indeed dazzling. Moreover to this project there seems no conceivable objection. It invades the rights of no one; apparently can do nothing but good. Neither we nor any other nation have a right to object to France erecting for herself a colonial empire in these uncivilized regions. By occupying and reclaiming them she will only work out the evolutionary edicts of progressive nature. Besides, the existence of a vast watershed in the desert will tend to its fertilization, if not to its

early repopulation, at the same time defining accurately the territorial limits of several nationalities. Cutting the necessary waterway will prove no light or costless task. When, some time ago the scheme was examined by a Government commission, it was shelved, the estimated expenditure fairly frightening official Paris. Then, when the gallant projector was plunged in despair, a good fairy appeared in the shape of the "universal separationist," who had but to touch with his wand the rock of French speculation, and the sixty million francs required were forthcoming. At least M. de Lesseps says that sum is provided. If all goes well, then, and the new report to be drawn up this spring should prove favorable, the current year may find operations commenced. Mr. Rouvaire considers that the lake, when made, will prove immensely valuable for commercial and military purposes. It will provide a new trade route in the direction of the African Sahara, also securing a strategic line for the defence of French conquests in Algeria and Tunis.

The Isthmus of Corinth is likewise doomed. General Türr and Count de Lesseps are completing their preparations for disconnecting the Morea. The short four miles of limestone rock, which now divide the "chafing waters" of Ægina and Lepanto, is likely soon to be cut in twain. As, happily, the intervening land between the two gulfs is nowhere more than one hundred feet high, this Periander-aged problem looks likely to be solved by the Nineteenth Century. What Nero subsequently failed in, the present may now be accomplished. That Emperor attempted the task, driving a 200 feet canal about half-a-mile inland from the western shore, near Kinchrea. His work, however, was stopped by the outbreak of rebellion in Gaul, which necessitated prompt and vigorous action, and ever since the scheme has rested, so far as active operations are concerned. Traces of this ancient channel may still be seen, but Time, the destroyer, has gradually filled in its bed, and the present depth is inconsiderable. As regards political influence, this waterway can exercise little or none. If it possesses any interest watever outside of Turkey and Greece, that interest will be confined to commerce. Even in this respect its value will be mostly felt by coasting traders. To vessels passing between ports on the Adriatic and the Black Sea, or beyond, it will doubtless prove of some utility, shortening by a couple of days or so, an inconvenient, if not dangerous passage. Athens, Salonica, and Smyrna will also benefit by a quickened route to the Italian ports; still it remains a moot question whether the canal will attract any sufficient percentage of the Euxine trade with places west of Marseilles, without which element it can scarcely prove remunerative. Natural seaways have one preponderating advantage over artificial ones. Nature demands no tolls, other than an occasional life, or shipload of cargo; whereas public companies require payment, frequently at exorbitant rates. Besides, there is a universal inclination to gamble, even when the dice played with are human lives. Many a foreign shipowner will prefer a longer and perhaps a more dangerous voyage to paying M. de Lesseps's company the heavy tolls it will certainly demand. Only a trifling gain will accrue to vessels bound to or from Gibraltar, or which touch at Malta, Algiers, or any other North African port. All trade between the Bosphorus and ports on the west coast of Italy must pass Messina, and to this again the saving will be but slight. Even the Messageries steamers which ply from Marseilles to Athens and Salonica, and whose route lies through the straits of Bonifacio and Messina, will scarcely find in the slight economy of time and distance, an adequate recompense for the extra charges which must be imposed.

Another scheme, quite as useful, if not more so, is to disunite the Crimea from the Russian mainland. Between Odessa and the grain producing districts of Azof and the Don there is a constant flow of commerce, and to merchants engaged therein the proposed northern passage will prove unmistakably valuable; while both Varna and the Danube ports will be brought into quicker communication with those fertile regions. On the other hand, it is very improbable that Kertch Strait will loose its present hold upon commerce with Constantinople and beyond; but it may happen, however, that in winter time, when Kertch is frozen in, a channel may be kept open through the canal, in which case the gain will be simply Euro-

pean; the question of a cheaper grain supply being evidently involved.

Probably, however, the Russian Government, when giving their sanction and authority to this project, were as much actuated by strategic as by commercial exigencies. With a second and thoroughly defensible exit, the Sea of Azof would become many times as valuable, if converted into a naval harbor. In its bewildering and unassailable recesses, a fleet of any given power might be collected—practically with impunity. Given money—which "bankrupt Russia," as they call her, can always find for military purposes — any number of leviathans might be built and launched almost unknown to us; certainly without our being able to interfere. Torpedoes and land fortifications could easily render the Perekof Canal and Kertch Strait impregnable; yes, even to attack from our latest types of steelclads. To blockade the hidden Russian vessels would require a couple of fleets instead of one; besides which both would have to be as powerful as that of the enemy, otherwise the blockading admiral would run a serious risk of being taken in flank by a navy circumnavigating the peninsula. Constructing the canal will also serve as a sort of guarantee against a renewed bombardment of Odessa. Any foreign fleet that essays the task thereafter will hazard destruction by a flank movement from Perekof, the vessels stealing out under the shelter of the Tendra peninsula. That this strategic highway will be cut, and cut shortly, scarcely admits of a doubt. To secure her empire, Russia will not stick at any trifling expenditure, and as a fact this sea-canal would make her southern dominions fairly secure against foreign attack; at the same time enabling her to re-establish her maritime position on the Black Sea, in defiance of the Treaties of Paris and London.

On the Lower Danube, Russia is equally active. She wishes to open the Kilia branch of that river to vessels of the largest tonnage by means of a deep sea-water canal, falling directly into the Euxine. Without entering into the diplomatic dispute whether Russia possesses any legitimate claim to the right bank of this outlet, the Danube navigation is a question demanding earnest attention on the part of Europe. Is it right for Russia (a power whose commercial policy is retrogressive, if not actively malignant) to be entrusted with the keys of this international highway? The Kilia mouth discharges a larger volume than either the St. George's or Sulina, its navigation is perhaps less difficult, and as a consequence it is more frequented by merchantmen. If the most important highway now, what will it be when the Russian seaway is cut? Will not its utility be enormously enhanced? Besides, there is always the contingency of the Danube Commission closing its labors, in which event the Sulina branch will soon become unnavigable and deserted. Trade will then be thrown entirely upon the Russian Canal—for the general good, or not? Admitting that a deepened outlet may, probably will promote the interests of Galatz, Ibraila, and other riparian towns, there is something intensely disquieting in the prospect of the dreaded Northern Power holding a firm grip upon this river. Heavy, perhaps prohibitive tolls are sure to be levied; fresh, or differential import duties may be imposed; additional taxes are likely to be laid upon exports, whilst even the new cutting may be subserviated to military expediencies and aggressive operations. This contingency, by the way, has been more than darkly hinted at by the *Golos*, *Viédomosti*, *Petersburskiaïa Gazeta*, and other semi-official journals. The Vienna correspondent of the *Daily Telegraph*, after expressing his belief that the Kilia Canal will make Europe the tributary of Russia upon the Lower Danube, and that this "will bring ruin to the riparian States, great and small," very properly remarks:

"It will sooner or later be discovered what an irreparable blunder it was to allow Russia to return to the Danube. In any case, England, whose flag covers two-thirds of the total navigation of the lower part of the river, cannot afford to be Russia's dupe in this matter, and will certainly not be led astray by any misstatement of facts connected therewith."

One other Russian enterprise deserves a word *en passant*. During the summer of last year (1882), the Government military canal at Cronstadt was opened for traffic, vessels with a draught of 14 feet successfully entering Lake Ladoga at low water.

Another bold separationist wishes tc

marry the Gulf of Saros with the Sea of Marmora, thereby making Europe independent of the Dardanelles. However, as a small mountain has to be cut through, this proposition may be placed by the side of schemes for bridging the channel or tunneling between Kintyre and Fairhead. Salonica, again, is unlikely to have an exit to the Gulf of Contessa, for some generations at least; but on the other hand, there is every prospect of Schleswig-Holstein being shortly divided. The distance to be excavated must be reckoned inconsiderable in these days of colossal undertakings, nor would the cost be extravagant, compared with the enormous benefits which Germany must obtain. The Skager Rack is frequently closed by ice during the long winters. At such times St. Petersburg, Riga, Stockholm, Dantzig, Kiel, Copenhagen, and other Baltic ports are shut out from all maritime communion with other ports. The proposed canal would greatly obviate this inconvenience, while, as regards the naval marine of Germany, it could not fail to exercise a re-invigorating effect. At present a navy is of little use to the Central Power; it can always be blockaded at the entrance to the Sound. But given a seaway out to the North Sea under cover of German guns, and the striking power of the Baltic fleet would be intensified tenfold. Moreover, the completion of this military and strategic outlet, would do more than anything else to lessen the German land-hunger in the direction of Denmark. To a puissant and aspiring ruler like the octogenarian Emperor of Germany, it must be extremely galling to find his superb creation shut in by the cannon of that small kingdom, which only preserves its independence through the jealousy of Europe. The existence of a trans-Schleswig ship-canal may be expected to allay in part the bitterness of this feeling, and its construction is purely a matter of time and opportunity. For the moment the enterprise is shelved, the Berlin Treasury having to find the "wherewithal" to execute another undertaking of more immediate necessity. From Bremerhaven to Bremen the Weser is to be made navigable for ocean-going vessels, and it will be a keen race between Manchester and the German town as to which shall first welcome a trans-Atlantic line. The event will be watched with some interest.

Three millions sterling will, it is estimated, be spent upon the Weser improvements, and the Government engineers promise conclusion in or about the year 1889. This intention opens up for Bremen the prospect of taking first place among the shipping centers of Germany; possibly, indeed, it may rival Berlin in point of population and splendor, as time goes on. Anyone who carefully studies the relative positions of Bremen and Hamburg, in connection with the European railway system, can hardly form but one conclusion, namely, that the former port, when open to ocean liners, will appropriate the chief trade with New York. This cannot but affect the future of Hamburg, and perchance, may seriously retard its progress. Germany, like France, is also paying increasing attention to its inland communications, several enterprises of great moment being now in progress in connection with the Elbe and Moselle. However, their importance is chiefly local and they can be dismissed without further notice here.

In Canada, the Welland Canal, connecting Lakes Erie and Ontario—of itself a splendid achievement—has been open to traffic for some time, and steamers of very considerable tonnage now pass from one lake to the other. However, this canal is but one section of a gigantic waterway which is intended to place the great Northwest in direct communication with Europe. Great efforts are now being made to deepen and reconstruct the various canals that now lead between Kingston and Montreal, and when a sufficient depth has been attained—uniform with that in the Welland channel—it will become possible for grain vessels to load in Manitoba and unship in Liverpool or London. Although this project attracts little attention here, it cannot fail to prove of immense consequences to the well-being of our colony and ourselves. Not the least advantage will consist in our consuming more Canadian and less American corn, and in the cheapening of our food supply, which must inevitably follow from this increased competition. In the second place, the Canadian route may appropriate to itself some proportion

of the rich trade centering in "Porkonopolis." According to a calculation that has been made, the distance between Chicago and Montreal by the Welland Canal system is 150 miles less than from Chicago to New York, by way of Buffalo and the Erie Canal. This fact, if true, should not be lost sight of, and alone might justify the enormous expense which Canada is now incurring in connection with this scheme.

In India and British Burma, millions are being spent on irrigation works and in improving the internal navigation of the Punjaub and North-western States. One colossal enterprise was opened in state last year, and another of almost equal value and proportions is drawing towards completion. However, the interest in these undertakings is chiefly local; few people in this country care a fig about the Sirhind, Bari Dôab, Soane, Orissa or Ganges Canals, and if the Indian language is to become "household" in this relation, it will be through a very different project. Probably few persons will call to mind at once the Island of Ramaiswaran or Ramisaram; it certainly has not earned for itself, so far, a gold letter page in the book of history, nevertheless, this *terra incognita* may yet win for itself a measure of fame not far inferior to that which has fallen to the lot of Nicaragua and Tehuantepec. Between India and Ceylon lie two islands, Ramisaram and Manaar, connected together by a sandbank commonly known as Adam's Bridge. Palks Strait divides the former from the Indian mainland, but the passage is dangerous and navigable only for small vessels; while the strait between Manaar and Ceylon is even less accommodating. It is now proposed to cut through Ramisaram and thus shorten the voyage to Madras, Calcutta, and the further East. This useful, non-ambitious, and realizable plan is under consideration by the Government of India, and it may, indeed be hoped that its execution may be sanctioned without delay.

Even Japan is afflicted with the canalizing mania, a bold project being on foot to dig a passage between Hamada and Hami, in the Island of Nipon; thus cutting off the dangerous point of Salo-no-Misaki or Cape Chickakoff, so dreaded by all mariners in these waters. It is proposed to adopt the Dutch system, and if commenced at all, the work will occupy at least three years, the distance to be cut being about ten miles. Japan certainly offers a wonderful opportunity to the marine engineer. Its population is as large as that of Great Britain and almost as dense; the people are wealthy, industrious, honest and intelligent, the revenue is not to be despised; coal and iron are plentiful, while the various sections of the Empire are separated by narrow straits. Here, indeed, is a splendid field for Sir Edward Watkin and his associates. By tunneling the narrow seas dividing Yesso, Nipon, Sikok and Kiusiu, no national interest can be endangered; the Japanese can only benefit. Besides this, the speculation would probably pay investers infinitely better than gold mining in India, as the want of some such connection is a standing grievance with the thirty-four million inhabitants of Japan. In shaping these islands nature has, perhaps, unintentionally adapted them to the requirements of the "separationist" as well as to the submarine "connector." The main islands want joining together, while all round the coast there are obstructive necks of land which might be beneficially dispensed with. New Zealand also affords a good field for marine engineering. The chief island is extremely long and narrow in places, presenting every inducement for successful partition. Two strips of land have been critically examined with a view to eventual operations, and before long a company may be started to give the chief city an eastern outlet.

Returning to the New World, we find Nova Scotia practically doomed, if not in the immediate present, at least in the near future. A similar fate seems in store for the odd-shaped Peninsula that now interposes itself between Cape Cod and Buzzard Bay, and which interferes with navigation between Boston and New York; while even the younger Continent is not without its schemes, unambitious ones though, it may be said. The natural configuration of Australia precludes our young colonies from fascinating their imaginations with any stupendous problems. Providence has proved too truly beneficent, giving Southern Australia a genial climate and tides that gently lave, scarce wash its shores. A rugged outline is therefore out of the

question; even deep indentations are rare. There are, it is true, a few promontories which may be severed for the general good, the approaches to one or two harbors may be improved, inland navigation may be attended to, but this is all. The great engineering feats which will stand as monuments of Nineteenth or Twentieth Century industry must be left to the older worlds to accomplish.

THE SELF-PURIFICATION OF PEATY RIVERS.

By W. N. HARTLEY, F. R. S. E.

From the "Journal of the Society of Arts."

Those chemists who are well acquainted with the classic researches of M. Pasteur, must have experienced a feeling of surprise when informed in the pages of the *Journal of the Chemical Society* that organic matter of a particularly stable character was oxidised and destroyed by the oxygen dissolved in water, under the influence of a comparatively low temperature. M. Pasteur has shown that, even at a temperature of 30° C., the oxygen of the air has but a very trifling action on extremely changeable material, such as the albumenoid matter in yeast water, or a solution of sugar. ("Annales de Chimie et de Physique," 3d series, vol. lxiv., pp. 35 and 36, also p. 71.)

Miss Lucy Halcrow and Dr. Frankland (*Journal of the Chemical Society*, vol. xxxvii., p. 506, Trans.) describe certain experiments, made with the object of testing Dr. Tidy's conclusions, that peaty water in the River Shannon loses more than 38 per cent. of its organic elements by oxidation, during a flow of only one mile. They contend that if peaty water possesses this extraordinary affinity for oxygen at ordinary temperatures, it cannot be necessary to appeal for proofs of it to large bodies of water, which are always liable, more or less, to alterations in the proportion of their organic elements from other causes.

They tried, first, the effect upon air of prolonged exposure of peaty water to daylight, but without agitation; second, the effect upon air of violent agitation with peaty water; third, the effect upon air of violent agitation with waters free from oxidizable organic matter. Their experiments lead to the conclusion that, if peaty matter dissolved in river water is spontaneously oxidized at all (of which they consider there is no sufficient proof), the process takes place with extreme slowness, and cannot be accomplished to any considerable extent in the flow of a river. The evidence proved the fact that peaty matter is less oxidizable than animal matters under the same conditions.

During these experiments, it was observed that a considerable precipitation of brown peaty matter occurred when the strong bog drainage was mixed with a comparatively small bulk of distilled water. This precipitation, it was observed, promised to throw light upon the amelioration of peaty waters which had been remarked by Mr. Bateman and other engineers.

In criticising Dr. Tidy's experiments on oxidation, Dr. Frankland ("On the Spontaneous Oxidation of Organic Matter in Water," Loc. Cit., p. 538) remarks on the apparently superior action that Dr. Tidy attributes to air acting on running water, which is absent in the case of falling water, unless it falls naturally in a river bed. The influence which so favored the oxidation of polluted water, running in rivers with numerous unpolluted affluents, appears always absent when the water is dashed into foam in a glass bottle, violently stirred up with glass rods, and especially when water is merely "exposed to light and air in a bottle."

Whether air has apparently a purifying action on river water, falling or running, superior to that exerted on water contained in a bottle, is a point which has not been proved by Dr. Tidy, though the means of proof are exceedingly simple. It appears to me, after a careful perusal of Dr. Tidy's paper, that all he has proved regarding the Shannon waters, is a decrease in peatiness during their onward flow. It is, therefore, an open question whether the self-purifying process is

due to oxidation. Indeed, Dr. Tidy himself states that, in the case of peat, the quantity of organic matter is kept in check by the following means, which are two, namely:

1. "The inherent power that water possesses of self-purification from the oxidation of the peat by the oxygen held in solution in the water. This process is enormously helped by certain natural and physical conditions, whereby the more complete aëration of the water, and the more intimate contact between oxygen and the peat, is effected."

2. "Mechanical precipitation by admixture with coarse mineral suspended matter. The artificial means of purifying peaty water are, storage, subsidence, and filtration."

We have, therefore, an alternative process of purification which is here termed "mechanical precipitation."

I am disposed to give every possible consideration to the question of oxidation, and am ready to allow that water exposed to air in a closed bottle, is always exposed to the same air, or, if the experiment be made in an open bottle, that it is not brought under the influence of fresh air except by the slow process of diffusion. Now, if it were proved that peaty matter in water was removed by aeration by *fresh air* and oxidation, we should be forced to the conclusion, sunlight being ineffectual, that the fresh air of the country contains some minute constituent, not present in sufficient quantity in the confined space of a bottle to make any perceptible difference in the water. Such an agent is ozone, which, in the atmosphere of the open country, never exceeds one volume in 400,000 of air. Although I have examined this question, I do not propose to offer any analytical data as evidence whether atmospheric ozone can, or may, act as an oxidizer of the organic matter of flowing waters; the evidence at my disposal shows that it does not. I mention this matter to show the train of reasoning which led to the following investigation. In the meantime, I shall only remark here that running waters are subjected to agents other than atmospheric air, which may, and do, sometimes act as purifiers. They are in contact with the river bed, and the soil on its banks.

River waters, as is well known, contain, besides peaty or other organic matter, certain mineral constituents, which are dissolved out of the river banks, or are the result of solvent action on the rocks which lie in the bed of the stream, or which compose the strata through which spring waters pass to feed the river. On the bed and the banks of the river, such solvent action may be expected to be most energetic where waters are dashed against the rocks in the act of precipitation over a fall. It, therefore, seems to me that the relation of the mineral constituents of streams to the self-purification of the water from organic matters, has scarcely been adequately studied by Dr. Tidy, notwithstanding his statements above quoted. I am led to this remark by certain observations of my own, made on different occasions during the last fourteen years.

In October, 1869, during a short residence in Ireland, I first observed that some peaty streams become rapidly decolorized, while others flow for a considerable distances without undergoing any visible alteration.

In August, 1874, while staying in Invernesshire, I remarked the course of the River Affaric, which flows from Loch Affaric through Loch Benevian, to join the River Glass, a run of six miles over a hard rocky bed (quartzite, micaceous schist, and basalt). No alteration in the color of the water was detected, for three weeks, while the observations though, were carried on, no rain fell, and there was abundance of sunshine. Several falls occur, and the stream is frequently lashed into foam along its course; it is, therefore, submitted to aeration in the fullest degree. This aeration is of the kind most likely to effect oxidation, the air being of the most highly oxygenated character, and charged with ozone. As the peaty coloration was not of a dark shade, any bleaching it could undergo would most certainly be noticeable. Nothing of the kind occurred, and although this "naked eye" inspection of the river was to my mind perfectly satisfactory evidence of the unaltered condition of the water, I do not wish it to be generally accepted as such. Certainly, nothing occurred resembling the bleaching process recorded by Dr. Tidy as having taken place at Doonass Falls, on the River Shannon, and the oxygenation of

the water was not productive of any visible result.

My experiments on peaty waters in Ireland, in 1869, showed that a certain amount of coloring matter was separated when a certain spring water of 26° of hardness was mixed with a soft peaty water. Furthermore, that the purification was much more effectual when the mixture was softened by an addition of lime water, as the calcic carbonate which separated carried down nearly all the peaty matter. The decolorizing power of aluminic sulphate was found to be such that two or three grains removed in twelve hours the whole of the peaty brown color from ten gallons of water. From this result, it was thought that certain clays, when mixed with water, would have a similar effect, and experiment proved this anticipation to be correct.

Mr. J. Y. Buchanan, formerly of the scientific staff of H. M. S. *Challenger*, in his investigation of the waters of highland lochs, found that the bottom waters were generally perfectly clear and colorless, while on the surface, and for several fathoms below the surface, water was colored with organic matter, and not always quite clear. When accompanying Mr. Buchanan in the steam yacht *Mallard* this fact was brought under my personal observation in Loch Ness, in places where the water is 50 to 110 fathoms deep, and the bottom consists of fine white or blue clay.

At the commencement of the year 1880, I felt that some information regarding the clearing of peaty streams would be valuable, and accordingly I placed my notes at the disposal of Mr. Gerard A. Kinahan, Associate of the Royal College of Science, Dublin, and advised him in the carrying out of this research in my laboratory.* I propose, therefore, to give in detail the methods of examination resorted to, together with the nature of the waters examined, the results obtained, and the conclusions arrived at; but, first, it may be advantageous if I define the propositions which are accepted. They are the following:

1st. Aeration by agitation in bottles is not effective in the purification of waters from organic matter. (Miss Lucy Halcrow and Dr. Frankland.)

2d. Some peaty streams become clear in the course of their flow, while others do not.

It remains for us to examine evidence of oxidation, or of other action, on the organic matter of peaty waters caused by natural aeration, and to investigate t influence of the river bed upon flowing streams.

The Effect of Natural Aeration on Peaty Waters.

In consideration of this question, the evidence available may be gathered in two ways, namely, by the "naked eye" inspection of rivers, and by analytical data concerning the organic carbon and nitrogen in the water. Many most important observations were made on small streams, but those yielding unimpeachable evidence were made on two rivers situated in the County Wicklow, in the course of which occur two very considerable falls of 360 and 700 feet respectively. The first of these is the Dargle River, which is precipitated over a rocky mountain-side, forming the well-known and beautiful Powerscourt waterfall. In October, 1881, on a fine, bright, and warm day, samples of water were collected. There was a slight flood on the river, and the water was unusually peaty, as during the two preceding days rain had fallen upon the hills; but, nevertheless, the water was free from turbidity. The first sample was taken a short distance above the fall, where the river flows through a deep channel in the mica-schist. From this point the water is precipitated over the face of the rock in a thin layer, presenting a remarkably large surface to the air. It is frequently dashed against rocks, and distributed in the form of spray, collecting again into a steady stream at the termination of its descent. It is altogether surprising to see how small a quantity of water is presented on so large a surface of rock. At the foot of the fall, separated 800 feet horizontally, and 360 feet vertically from the channel before mentioned, the second sample of water was collected. No visible drainage of any kind enters between these two points. The long range of mountains constituting the drainage area of the River Dargle, above the fall, is principally composed of granite, the

* Report on the Clearing of Peaty Waters. Second eries, vol. iii. Proc. Roy. Irish Academy, pp. 447, 596.

mica-schist occurring only at a short distance from where the descent commences. Peaty matter is distributed over nearly the whole area. When the two samples were examined in the laboratory, in a vertical tube eighteen inches long, there was no visible difference between them. Both showed a clear dark-brown color, with very little suspended matter, and deposited only a slight brown sediment after standing.

The analysis of these waters was made in the following manner: A litre of each sample was evaporated down to 100 cc. in flasks, the complete expulsion of the water being accomplished in a glass dish, under a glass shade of the form employed by Dr. Frankland. All the precautions observed by Dittmar and Robinson were attended to, and the residue was burnt in a combustion tube open at both ends, the anterior portion of which was plugged with silver wire. In burning the residue in an open tube, the air necessary was very carefully purified by being collected in a glass gas holder charged with diluted solution of caustic alkali. As it was discharged, it passed over caustic alkali, in sticks, through a soda-lime tube, and through two bottles containing oil of vitriol. The current of air was maintained as a steady stream by the pressure of six inches of water, glass taps being used to regulate the pressure.

It is important that no india-rubber joints be used except where absolutely necessary, the air being exposed as little as possible to the india-rubber. The water residue was removed from the basin by a platinum spatula, and the final portions were detached from the glass, by rubbing the surface with some granulated copper oxide. The water residue, after being transferred to a boat of platinum recently made red-hot, is covered with copper oxide previously ignited. The gases from the combustion, passing out of the tube, enter first a small U-tube containing a 50 per cent. solution of sulphuric acid saturated with chromic acid, then a small tube containing fused but porus calcium chloride, pounded into very small pieces, and sifted free from dust. The carbonic acid was collected in a soda-lime tube, the soda-lime being separated by a plug of glass wool from a layer of calcium chloride. These solid absorbent materials were all pounded and sifted, so as to pass through meshes of wire gauze with about 400 holes to the square inch, all dust being separated by a finer sieve. The weighings were made on a balance capable of turning easily with the 1-10th of a milligramme. The nitrogen was estimated by burning the water residue with soda-lime, sifted as before, in a copper boat, in a tube through which passed a stream of perfectly pure hydrogen, incapable of giving any reaction showing presence of ammonia when passed through Nessler solution. It is important that the hydrogen be carefully tested, since it has been found to yield ammonia after very careful purification with solid and liquid reagents, capable of absorbing the alkali. The same supply of gas was found not to contain ammonia after it had been strongly heated in a glass tube. Of course, the presence of nitrogen oxides in the sulphuric acid would fully account for the formation of ammonia, by the action of nascent hydrogen disengaged in the apparatus, but it is scarcely conceivable that this could pass out of the acid liquid in which it would be formed. The apparatus, however, was of the form known as Kipp's, and if the action be rapid and the gas accumulates in the central bulb, the zinc becomes almost dry. Under these circumstances it is conceivable that ammonia might be carried off by the current of gas; but it is scarcely credible that it should pass uninterruptedly through a very efficient drying apparatus charged with oil of vitriol. In all cases precautions were taken to ascertain that no source of error should arise in this way, by making blank experiments previous to commencing the actual analytical operation. The analyses were made in duplicate, and in the weighings for the organic carbon the allowable error was taken as under three-tenths of a milligramme of carbon dioxide—or less than one part of carbon in ten millions of water. Nitrates and nitrites were estimated, the quantity was that merely occurring in rain-water; and as we are not concerned either with these constituents of the waters, or with chlorine and solids, I need not describe further the analytical process. The results obtained from the analyses of the water above and below the Dargle waterfall, are stated in the following table:

ANALYSIS OF WATER FROM THE DARGLE RIVER, ABOVE AND BELOW POWERSCOURT WATERFALL. THE CONSTITUENTS ARE STATED IN PARTS PER 100,000 OF THE WATERS.

	Above the Fall.	Bel'w the Fall.
Organic matter :—		
Carbon	0.946	0.944
Nitrogen	0.072	0.077
Ammonia	0.001	0.001
Nitrogen, as nitrates and nitrites	..	..
Total combined nitrogen	0.073	0.078
Chlorine	0.48	0.88
Total solid matter	4.20	4.40

REMARKS.—Clear, but deeply colored with peaty matter, October 1st, 1881.

The proportions of organic carbon and organic nitrogen, under the circumstances of such effective aëration as a fall of 360 feet in a vertical direction, and for most of the distance in the form of spray, should undergo some alteration if oxidation is possible by aeration. The two constituents in the two samples show no further variation than may be fully accounted for by experimental error, namely, merely two parts of carbon and five parts of nitrogen in 100 million parts of the water.

The next water examined was that of the Carawaystick brook, which flows out of a small sheet of water called Kelly's Lough, situated on the south-eastern slope of Lugnaquilla, at an elevation of 1,700 feet above the sea. The descent of this stream is rapid, for in a flow of less than two miles the water runs at a level lower than Kelly's Lough by 1,200 feet. At one part of its course it is precipitated into the valley of Glenmalure, 700 feet below, falling over a bed of granite and schistose rock in a series of foaming cascades of nearly perpendicular descent, and exceedingly picturesque in appearance.

Aeration and agitation are here as complete as possible. In no part of its course is any diminution in the peaty brown tint of the water visible, though a most careful comparison was made of the waters above with those below the falls.

Samples of this water, collected both in summer and winter, were taken at a distance horizontally of 1,600 feet apart, the vertical height between the points being 700 feet. No side drainage enters with the fall, nor is the water submitted to any action or change of condition, other than complete aeration, which can possibly effect the organic constituents. In January, 1882, when the winter samples were bottled, there was a light mist on the hills, otherwise the day was fine and dry, but not cold. The waters were clear, and of a light olive brown tint. The samples collected in summer, August 15th, 1881, were deeply colored with peaty matter. The streams are generally browner in summer than in winter, and in frosty weather they are often quite colorless. A table, showing the results of these analyses, follows:

ANALYSES OF WATER FROM THE CARAWAYSTICK RIVER ABOVE AND BELOW GLENMALURE FALLS.

IN 100,000 PARTS OF WATER.

	Organic matter.		Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Chlorine.	Total solids.	Remarks.
	Carbon.	Nitrogen.						
Samples collected in Winter.								
I. Above falls	0.284	0.022	.....	0.004	0.026	.....	.....	Clear, but colored with peat. Collected Jan. 7, 1882
II. " "	0.286	0.025	.....	0.004	0.029	1.5	3.26	
III. Below falls	0.284	0.021	.. ...	0.004	0.025	1.6	3.34	
IV. " "	0.289	0.025	.....	0.004	0.029	.....	.....	
Samples collected in Summer.								
V. Above falls	1.06	0.054	trace	.....	0.054	1.30	.....	Deeply col'd with peaty matter.
VI. Below falls	1.17	0.053	trace	.....	0.053	1.4	.. ..	
VII. From Kelly's Lough	0.68	0.054	trace	.....	0.054	1.2	.....	Rather peaty. Collected Aug. 15, '81

The composition of the water above and below the falls is almost identical; there is no decrease in the amount of carbon and nitrogen, which shows that aeration of the most effective character has no effect of the slightest kind in decreasing the organic constituents. An analysis is added of the water from Kelly's Lough.

I consider the foregoing analyses conclusive evidence, that a peaty river water cannot undergo the slightest degree of purification from its organic constituents, by the natural process of aeration.

I now come to the consideration of the altered conditions in a river water, caused by the reception of a mineral matter of a more or less soluble character.

The Mechanical Action of Insoluble Matter is Devoid of Decolorizing Power.

It was found by experience that the depth of color of peaty water was a fair indication of the quantity of organic matter present, and the following evidence is dependent on this fact:

(1.) At the point where samples of water were taken out of the stream which flowed from Kelly's Loch, another series was collected under the following circumstances: The first came out of the river at the foot of the falls, then at the head of the falls a quantity of sand clay, about a cubic yard in measurement, was thrown into the stream; just above this point the second sample was taken. The whole of the water was rendered very turbid. A descent was then made to where the first sample was taken. After waiting until the turbid water had flowed for some time, a third sample was taken. On comparing the color of these waters, after the turbidity of the last had subsided, it was found that the first and second showed no difference in color, while the third was the darkest.

The following materials were added in large proportions to peaty waters, and after thorough mixing by violent agitation in tall cylinders, the mixtures were allowed to subside. Any diminution in tint of the water was noted by comparison with the original sample.

(2.) A pure quartzose sand, even when added in large quantities to the water, had no perceptible action on its coloring matter.

(3.) Pure gelatinous silica, which acts to some extent as a mordant of aniline dyes, was quite without effect.

(4.) Magnesia was without action.

(5.) Pure carbonate of lime has only a very slight action on the peaty coloring matter.

(6.) Powdered chalk and limestone were found to reduce the tint slightly.

(7.) Clay of different kinds, which had been treated with a seven per cent. solution of hydrochloric acid, so as to dissolve out the soluble matter was found to have only a very slight decolorizing power.

The results of the experiments described as Nos. 1. 2, 3 and 4, prove that there is no decolorizing action on the peaty coloring matter which can be described as *mechanical*. Pure carbonate of lime, powdered chalk, and limestone, are slightly soluble in water, and it is a chemical action of these substances which reduces the peaty tint. The action of the insoluble constituents of clay appears likewise to be due to the iron and alumina they contain, to which the peat coloring matter attaches itself as to a mordant.

The Agents Operating in the Self-Purification of River Waters.

The decolorization of peaty streams by mine drainage.—The Caraway stick brook, running down into Glanmalure, joins the Avonbeg. This river, uniting with the Avonmore at the "meeting of the waters," forms the Avoca River. The changes which occur in the Avonbeg are irregular; but there is, in general, a diminution of the peaty tint.

The Avoca River, however, in the course of its flow, soon receives on its east bank waters from the mines of Tigroney, Cronebane, and Connary, which waters contribute to the streams ferrous sulphate and alumina sulphate in comparatively large quantities, while smaller proportions of such substances as copper sulphate, and arsenic, are found therein. On the west bank of the river, but lower down, are the Ballymurtagh and Ballygahan mines. A sample of water, taken at the tail of the landers, at Ballygahan, there were found 56.8 grains of ferrous sulphate, and 54.7 of alumina sulphate per gallon. This water is capable of removing, in a most complete manner, the coloring matter from fifty times its volume of very peaty water. A dark brown precipitate first settles, and then a deposit of ochre occurs.

The small tributaries of the Avoca River contain considerable quantities of

dissolved mineral matter, as shown by the following analyses:

ANALYSES OF THE WATERS OF THE SMALL TRIBUTARIES OF THE AVOCA.

	Parts per 100,000 of the water.	Remarks.
"Red Road" Stream.	Ferric oxide. } Alumina.... } 3.8 Copper 0.05	Colorless, slightly acid, ochre in suspension.
Tunibinch Stream.	Ferric oxide. } Alumina. .. } 4.8 Copper .....0.02 Total solids..12.8	Colorless, slightly acid, brown sediment.

A sample of slightly turbid but very peaty water, taken from the Avoca River, below Ballygahan mines, deposited a brown precipitate after being at rest for some time, and the coloring matter was much reduced. On another occasion, the tint of the water was not so deep, and after settling, the color was quite removed. The precipitation of the peaty matter is followed by a deposit of ochre.

When a few drops of a solution of ferrous sulphate are added to a quantity of peaty water, a peculiar turbidity is the result, this afterwards increases to a brown precipitate, which collects at the bottom of the vessel, the water being free from peaty coloring matter.

It is very noticeable, in the Avoca River, that the stones are covered with an ochreous deposit, and that ferruginous matter is deposited in the pools. It appears that the ferrous salt in solution carries down the organic matter at the time it is oxidized, for this is the action observed when a pure solution of ferrous sulphate is added to peaty water.

Samples of water were collected on the 16th January, 1882, at Tigroney Weir and Black Dog; between these points, the Avoca River falls only fifty feet in a flow of three miles, and therefore aeration, which, *per se*, has been shown to be without action on the organic matter in the waters of the Carawaystick Brook and the Dargle River is certainly inoperative in this case.

The subjoined analyses of these samples show that with the increase in the mineral matter there is a decrease in the organic matter held in solution:—

ANALYSES OF WATER FROM THE AVOCA RIVER, BEFORE AND AFTER RECEIVING MINE DRAINAGE.

PARTS PER 100,000 OF WATER.

	Organic matter.		Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Chlorine.	Total solids.	Remarks.
	Carbon.	Nitrogen.						
I. At Tigroney Weir	0.231	0.026	trace	0.011	0.037	1.8	4.88	Slightly peaty.
II. " "	0.229	0.028	trace	.....	0.039	.....	.....	
III. At Black Dog...	0.098	0.019	trace	0.008	0.027	2.3	9.26	Colorless.
IV. " " ...	0.093	0.019	trace	.....	0.027	.....	.....	A trace of iron present Collec'd Jan. 16, 1882.

The effect of low temperature on dissolved peaty matter.—Mr. G. A. Kinahan remarked that, when there was a considerable quantity of snow on the hills forming the drainage area of the tributaries of the Avoca River, the peaty color of the waters was diminished much below that of ordinary occasions, and, indeed, the waters frequently were quite colorless. Mountain streams presented the greatest difference in appearance in frosty weather; some which were usually deep brown in color became perfectly clear and free from color during frost.

When peaty water is frozen in tall cylinders, a layer of deeply-colored water collects at the bottom, and the water resulting from the thawing of the ice is quite free from color. If the freezing takes place from the sides of the vessels, a core of peaty water collects in

the center. When freezing occurs with great rapidity, a mass of clear ice is formed, in which are liquid enclosures of concentrated peaty water.

When freezing proceeds in a cylinder from above downwards, a clear and colorless block of ice may be removed, and a layer of deeply colored water is left at the bottom. Repeatedly filling up with peaty water, and again freezing, causes the formation of a very small quantity of a brown sediment. It does not appear from this that freezing causes a precipitation of peaty coloring matter in an insoluble form, though it does cause a purification of the water frozen, and a concentration of the peat solution. As, however, it was the water and not the ice of the streams which was under observation, there must be some other cause for its greater purity during frost.

Mr. Kinahan explains it in the following manner:—In frosty weather the surface of the bogs is frozen, preventing the percolation of waters which thus run away clear; and the water supply is often derived from melting snows, which are, therefore, uncontaminated. Thus, in one case, it was noticed that though a quantity of peaty water had collected behind a snowdrift, yet the water flowing from the other side of the drift was quite colorless.

On the Chemical Action of Clays, or the Mineral Constituents of Soils, in Purification of River Water.

From my opening remarks, my views, as to the action of clays in the self-purification of river waters may be gathered. In order to ascertain how far the mountain streams of the county Wicklow are actually, or may possibly be, purified from organic matter by the chemical action of the clay in the bed or on the banks, specimen of clays and disintegrating argillaceous rocks were collected for examination.

Experiments were made by shaking up weighed quantities of the clays with carefully measured volumes of water, contained in tall cylinders. When the water had become clear, a large portion was decanted or syphoned off, and placed in another cylinder, for comparison with the color of an equal volume of the original water. By this treatment, it was found that, provided only a sufficient quantity of the clay was used, any peaty water could be decolorized.

I will now give the description and the analysis of the clays collected by Mr. Kinahan, together with an account of their action.

No. 1.—A fine cream-colored clay, containing fragments of iron pyrites and milk quartz, was found to be a most efficient purifier. When added to peaty water, the greater portion settled down almost immediately, but the very fine particles remained in suspension for some time. Complete subsidence left the water very clear and colorless, when sufficient clay had been used; the sediment was covered with a brown layer, as if the coloring matter was there deposited. With a liter of dark peaty water, 4 grammes of clay rendered the water clear and colorless; 2.5 grammes caused destruction of the color, accompanied by persistent turbidity, which was not apparently decreased by filtration; 1.5 grammes changed the brown color to olive green.

The clay was shaken up with water, in the proportion of 10 grammes of the former to 300 cc. of the latter; the liquid was filtered, and the filtrate was found to possess the property of decolorizing peaty water. Tho clear solution has an acid reaction, and contains a small quantity of ferrous and alumina sulphates. The insoluble portion of the clay was digested with hydrochloric acid, and well washed. Its action as peaty water was scarcely perceptible after this taeatment.

Its composition was as follows:—

Analysis.	Per cent.
Insoluble in hydrochloric acid	92.51
Ferric oxide	2.35
Alumina	2.43

No. 2.—A disintegrated steatic shale of a light yellowish red color. It yields on treatment with water, a turbid yellowish liquid, which clears on standing, the suspended particles subsiding; the resulting clear liquid, which is neutral to test paper, contains very little dissolved matter. Only a very minute inorganic residue is left after digesting 5.5 grammes of the clay with water, filtering and evaporating the filtrate to dryness.

Its composition :—

Analysis.	Per cent.
Insoloble matter	78.28
Ferric oxide	8.12
Alumina	7.96
Manganese	trace

In a liter of water, 25 grammes caused perfect purification. Insufficient additions of the clay caused a persistent turbidity, or an olive green color, as in the previous case.

No. 3.—A bluish clay occurring in a cliff on the Ballynagappoge brook. When added to a brown peaty water, it removes all but the green tint, and leaves a slight turbidity. It forms a dirty turbid liquid when mixed with clear and colorless water, and a peculiar turbidity to some extent remains after the liquid has been left some time for subsidence to take place. This turbidity, which resembles that of imperfectly purified peaty waters, is found to be removed by an addition of a small quantity of a red clay about to be described:—

Analysis.	Per cent.
Insoluble portion	81.79
Ferric oxide	6.72
Alumina	6.47

No. 4.—A brick-red clay from one of the tributaries of the Mucklagh brook. With pure water, this clay yields a perfectly clear solution after the fine particles have subsided. From a liter of water, 20 grammes of clay removed all the peaty matter. It is very efficient in purifying action, but insufficient quantities of the material cause the before-mentioned changes in the water, from brown to olive-green, or else a turbidity which, from the color of the clay, makes the water appear of a red color.

Analysis.	Per cent.
Insoluble matter	72.41
Ferric oxide	9.70
Alumina	9.41

Mr. Kinahan has remarked that the blue clay, No. 3, seems to have been originally a red clay like No. 4; its present condition being due to percolating peaty waters. I will quote what he says on the matter:—

"1st. A similar blue clay overlies No. 4, as seen in the banks along the Mucklagh brook, the passage from one to the other being gradual; red stones and particles of red clay are seen occurring in this over-lying clay.

"2nd. The residues left by both specimens, after digestion in hydrochloric acid, are alike.

"3rd. The percentage of insoluble matter in No. 3 is greater than in No. 4, the more soluble portions, iron and alumina, having been probably removed by percolating waters."

No. 5.—A whitish sandy clay from the banks of the Carawaystick brook, where it occurs in an ancient moraine above the falls. It was this material which was thrown into the stream above the falls, and found to have no perceptible action as a decolorizer. In the laboratory this result was confirmed; even when added in very large quantities, it produced very little alteration in the color of peaty water.

Analysis.	Per cent.
Insoluble matter	93.91
Ferric oxide	1.63
Alumina	2.17

No. 6.—A disintegrating granite, from Aughavannagh, county Wicklow, a rock which underlies the peat of the district. Its constituent minerals are quartz, black and white mica, and felspar, passing into china clay. This specimen acts very slowly, the impurities in the water settle out in a layer after the mineral has been shaken up with the water, and repeated shaking causes a marked reduction in color. The felspar, which is a very hard variety containing very little iron, acts in the same way. It was noticed that the layer of water lying next the mineral for some time in a glass cylinder, was less colored than that above.

Analysis.	Per cent.
Insoluble matter	86.68
Ferric oxide	3.80
Alumina	6.24

The turbidity which is mentioned as occurring after the peat-coloring matter is removed, is due to the pedetic motion of the fine clay particles. Such turbidity is at once removed by an addition of saline matter in solution, as, for instance, common salt, or sea water, or, as is shown here, by an excess of clay. (For further particulars regarding this clay turbidity, see the *Quarterly Journal of Science*, April, 1878, vol. viii., "On the Movement of Microscopic Particles Suspended in Liquids," by Prof. W Stanley Jevons, F R.S.; also in connection with this subject, "An Explanation of the 'Brownian' Movement," W. N. Hartley, *Monthly Microscopical Journal*, June, 1877.)

The Action of Metallic Hydroxides and Oxides.

It was thought desirable to ascertain the action of commonly occurring forms of metallic oxides as purifiers of peaty water, the method of testing being the same as that carried out with the clays.

The coloring matter of peat was rapidly and efficiently precipitated by aluminic hydroxide, ferric hydroxide, and manganic hydroxide. Alumina is by far the most active, the iron compound coming next. The oxides of these metals were much less rapid in their action. Only after repeated shaking with the water did the manganese dioxide and ferric oxide produce any perceptible alteration in tint.

Alumnic hydroxide was mixed with water, and the solution filtered. The filtered liquid caused percipitation of the coloring matter from peaty water. Hydroxides of iron and manganese, submitted to the same treatment, yield solutions which are quite inactive.

The Action of the Clay in the Bed and Banks of a Stream upon the Water which flows against it.

As an evidence of the active nature of a river's bank as a means of purifying the waters, a careful observation, made by Mr. G. A. Kinahan, has shown that the "Ballynagappoge brook, the head waters of which are deep brown, and the feeders of which are equally peat, is purified to a great extent by the blue clay, No. 3, which forms a cliff on the banks of the stream, and against which the waters wash and remove therefrom a considerable quantity of the mineral." Clay No. 3, has been proved to be a most efficient purifier, and after contact of the waters therewith, the stream is seen to be very slowly but steadily reduced in color. The waters fall about 500 feet in a distance of a mile and a-half, the stream flowing rapidly in a succession of little falls; at about five hundred yards above Rosahane bridge, the rate of fall is diminished, and the rocky bed is changed to one of clay and gravel. The peaty matter is here reduced to a mere trace. Just above the bridge, where the fall is slight, there are several marshy places from which iron stained waters flow into the brook, and below the bridge the water is completely decolorized. The ferrous carbonate contained in the waters from the marshy land, yields ochreous precipitations of peaty matter, and renders the water most beautifully clear and colorless. The ferrous salt is produced by the action of water and decomposing organic matter upon the iron compounds in the clays and gravel. The deposits of ochre on the stones and in the pools of the streams, resemble those caused by the mine drainage waters discharged into the Avoca River. This is a true case of the self-purification of a river water by the action of a mineral constituents contained in its bed and banks.

From a number of experiments made on peaty coloring matter in water, I conclude that it acts like an organic acid, and that it is probably a body of the type of alizarine or litmus, being only slightly soluble, or even insoluble, in pure water, but readily dissolved in water containing traces of alkili, or of soluble carbonate, such as ammonia or potash. With metallic oxides, iron and alumina, it forms insoluble compounds of the nature of "lakes." Lime water also precipitates it. Mineral acids, sulphuric, hydrochloric, and nitric, precipitate it. Peaty water may be perfectly bright, and free from turbidity. These facts, and a further observation that subsidence will not clear a peaty water of its coloring matter, lead to the conclusion that the coloring matter is held in solution, and precipitated as a lake by various mineral bases.

On Dr. Tidy's Conclusions as to Natural Oxidation of Peaty Matter.

On page 295 of the Chemical Society's *Journal*, vol. xxxvii., appears the following passage ("Tidy on River Water"):—

I have had opportunities, during a fortnight's inspection professionally, of studying this subject in that most wonderful of great rivers, the Shannon, along a run of very nearly fifty miles (from Portumna to Limerick), the river receiving, throughout the whole course examined, not only feeders containing—except in one or two cases—an even larger quantity of peat than itself, but the drainage, of a deep coffee tint, and in not inconsiderable streams, from huge bogs covering many square miles of country. I am aware that I am now entering upon a subject where great differences of opinion exist, and I would approach it cautiously.

Let me, then, put the case of the Shannon below Portumna, containing a quantity of peat, represented by 0.9 of organic carbon per 100,000 (for in arguing this question of oxidation I shall, in the main, appeal for manifest reasons to the indications of the combustion process), receiving, as it continues to flow, an enormous quantity of black peaty matter, and streams much more peaty than itself, and drainage from an area that can only be described truly as a peat bog. Should we not expect, I ask, if the water contained in itself no inherent power of purification (I mean, of course, the power of effecting oxidation as the peat), that a sample examined thirty-five miles further down the river, say at Castle Connell, would contain an enormously increased quantity of matter? But the reverse of this is the case. For not only does the two-foot tube tell that the peat is manifestly less, but on analysis the organic carbon at Castle Connell is found to be only a little over one-half what it was at Portumna. What then has become of it? Certain it is the peat could not have evaporated, and I confess I can invent no possible explanation of its disappearance except by believing that, in the course of its flow, it has been oxidized by the oxygen held in solution by the water. At any rate those who hold non-oxidation in running water must explain how it is that the absolute organic carbon, by a flow of thirty-five miles (even in the face of the fact that the river in the course of its flow is receiving enormous volumes of highly peat-charged water) is lessened in so remarkable degree.

Samples of water were taken from either ends of Lough Derg, through which the Shannon flows; the length of the lough in twenty five miles. The difference is the organic carbon of these two samples was only 0.18 parts per 100,000.

A third sample was taken one mile below Killaloe. As a matter of fact, there is less bog drainage going into the river in this short run than at any other spot in the fifty mile flow examined, although even here the quantity that finds its way into the river is not inconsiderable. Nevertheless, in the course of this one mile flow, the organic carbon fell from 0.8 to 0.48. Incidentally, I may mention, that these results were confirmed by the oxygen process, and by the appearance of the water in the two-foot tube. From this point to within half-a-mile from Castle Connell, the quantity of peat drainage that enters the river is enormous; indeed, an extensive peat bog, covering miles of country, pours into the river its absolutely black drainage water in full-sized streams. At O'Brien's-bridge, the organic carbon was found again to have risen to 0.84 per 100,000, and I have no doubt, from the two-foot tube observations, although I have no analysis in proof, that below this spot the quantity of organic carbon would have been even greater. This highly peaty water, after a very short run indeed, although (and I must again insist on this point) here and there receiving notable quantities of bog drainage, becomes very manifestly less peaty, the organic carbon being reduced from 0.84 to 0.593 per 100,000 parts.

How can this remarkable result be accounted for? Thus, between the two points where these samples were taken, are to be found the far-famed falls of Castle Connell, where, in the course of a few hundred yards, the level of the river sinks fifty feet. The extent of aeration the water must undergo in these magnificent falls must be considerable. The part they play in effecting the purification of the water is strikingly manifest, in comparing the dark tint of the water as it foamed and bubbled over the rocks with which it first came into contact in the upper part of the Doonas Falls, with the comparatively light peaty tint the water exhibited as it played over the rocks situated at the lower parts of the falls. I watched these changes of tint for some hours along the course of the falls, and a more magnificent natural experiment I never witnessed. If this alteration in actual quantity, as shown by analysis, is not oxidation, I am totally at a loss to conceive what it can be.

I have quoted Dr. Tidy's own written words, in order that I may not in any way misrepresent his opinions, or the nature of the evidence upon which he founds his doctrine of the oxidation of peaty matter in running water. The organic carbon at Castle Connell was found to be only one-half what it was at Portumna, thirty-five miles further down the Shannon. Let me draw attention to the following words:—

> Certain it is the peat could not have evaporated, and I confess I can invent no possible explanation of its disappearance except by believing that, in the course of its flow, it has been oxidized by the oxygen held in solution by the water.

It will be remembered that, at Castle Connell, the river sinks a trifle of 50 feet in the course of a few hundred yards, and the water collected by Dr. Tidy below the falls contained less organic carbon than that collected above. "If this alteration in actual quantity, as shown by analysis, is not oxidation, I am totally at a loss to conceive what it can be." Shortly afterwards, we are told, p. 300—

> These, then, in the case of peat, are the natural means whereby the quantity in running water is kept in check.
>
> 1. The inherent power that water possesses of self-purification from the oxidation of the peat by the oxygen held in solution in the water.

I have already shown the most complete aeration, effected by a descent over falls 360 and 700 feet in height (not a mere 50 feet), is quite without action—that there is no oxidation of the peaty matter.

The evidence is directly contradictory to the above positive statement.

How then are we to account for the diminution in organic carbon, as traced in the flow of these peaty streams? The question admits of a very simple explanation. Mr G. A. Kinahan visited the Doonass Fall on the Shannon, at my desire and collected samples of water, on both sides of the river, above and below the falls. His analyses showed that there was a greater difference between the two samples from opposite banks, above the falls, than between either of these samples and those taken below the falls. Moreover, the organic carbon was found to be increased in quantity in the samples collected after the water had descended the falls.

A few bottles full of water taken out of a very large river, here and there, are not average samples of the whole water in the river, even when the greatest care is exercised in collecting the samples.

The following remarks from Mr. G. H. Kinahan, of the Geological Survey, who lived for many years on the banks of the Shannon, in the neighborhood of Doonass Falls, and for part of the time at Castle Connell, warrant this statement; and, at the same time, fully account for the discrepancies in the results obtained by Dr. Tidy and Mr. Gerard A. Kinahan.

In the Shannon, between Killaloe and Castle Connell, the flood waters from the counties of Tipperary and Limerick, on the south-east and south, are 'black floods' (peaty water), especially those that flow into it between O'Brien's-bridge and Caslte Connell; while the flood waters from the Clare side, to the north and north-west, except the stream from O'Brien's-bridge bog, are 'red flood,' highly charged with the red muds from the *débris* of the red basal carboniferous shales. These different classes of flood may differently affect the water at the fall of Doonass. If rain falls only in the counties of Tipperary and Limerick, there will be a 'black flood' over the falls, while, if the rain falls only in the county Clare, it will be a 'red flood;' but if the rain is falling on both sides the Shannon, the results will be very different. If, during such a rainfall, you stand at the World's End Weir, Castle Connell, the flood on the Limerick side will be black, and that on the Clare side, red, the two differently-colored waters going separately over the weir, to be slightly mixed below; bnt the great mixing does not take place until they reach the Falls of Doonass, below which the red ferriferous waters are found to have cleared out the peaty coloring matter. The waters flowing over the falls are more often colored with peat than otherwise. While living at Castle Connell, some of the largest floods I saw on the falls were black ones. When the rain falls only to the southward of the Shannon, the 'black flood,' going over the fall is met by a 'red flood' coming out of the Annacotty, or Mulkear River, which neutralizes and destroys the peaty color in the water before it reaches Limerick. On account ot the land on each side of the Shannon, above Castle Connell, the waters on each side of the river above the falls must give very different analyses.

These remarks are in accordance with the observations made on the clearing of the peaty rivers of the county Wicklow by the action of clays and soluble mineral matters.

The analyses of the waters collected immediately above and below Doonass Falls are the following:—

ANALYSES OF WATER FROM THE RIVER SHANNON. THE SAMPLES WERE COLLECTED FROM THE RIGHT AND LEFT BANKS, ABOVE AND BELOW DOONASS FALLS.

PARTS PER 100,000 OF WATER.

	Organic matter.		Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Chlorine.	Remarks.
	Carbon.	Nitrogen.					
Above Falls.							
I. From right bank.....	1.036	0.053	0.004	trace	0 057	1.75	Slightly peaty and colored with suspended matter.
II. From left bank.....	0 818	0.040	0.002	trace	0.042	1.70	
Below Falls.							
III. From right bank.....	1.063	0.049	0.002	trace	0 050	1.77	Collected July 16th and 18th, 1881.
IV. From left bank.....	1.090	0.045	0.001	trace	0.046	1.87	

Dr. Tidy's results, which I conjecture were obtained from an examination of the waters on the Limerick side of the river only, above the falls, may be easily reconciled with the fact that the lessening of organic matter is not caused by oxidation. When we consider that the ferruginous mineral matter, free from bog waters, is discharged on the Clare side, that when mingling with the bog drainage it precipitates the peat, that these waters when flowing on a level must, to some extent, mingle, but when descending falls must be more or less mixed, then the phenomenon witnessed at Doonass Falls is capable of easy explanation. It is nothing more than the mixing of two waters, followed by a precipitation of organic matter contained in one of them.

I have here made no allusion to the question of oxidation in sewage-polluted streams; that has already been treated of by Dr. Franklin. He shows that a flow of between eleven and thirteen miles of a polluted stream has very little effect on the organic matter dissolved in the water, even at a temperature of 18° C. And he has shown that the case of the River Wear, flowing between Bishop Auckland and Durham, which has been quoted by Dr. Tidy in illustration of his theory of oxidation of sewage, the purification is caused by an admixture of highly ferruginous waters, a fact which does aot appear in Dr. Tidy's quotation. I have felt it desirable that facts concerning the possible oxidation of peaty matter in water by natural means should be laid before the Society, and that the cause of the amelioration of such water, which has been not unfrequently observed, should at least receive some explanation in harmony with all hitherto ascertained facts.

Discussion.

The Chairman in inviting discussion, remarked that the paper was confined to the self-purification of peaty waters, and it would be well, therefore, not to travel beyond that subject.

Dr. Voelcker said he felt personally indebted to Professor Hartley for the facts he had brought forward, inasmuch as they fully confirmed the view he had long entertained. At least thirty years ago he had worked with Professor Mulder on the humic acid series, and found it exceedingly difficult to effect oxidation of humic or ulmic acid. The view he had long entertained was that the removal of peaty matter from water was due to a separation of these organic acids of the humic series by mineral matters, especially basic iron salts, aluminic salts, and in a minor degree, by lime salts. The color of peaty water is due sometimes to very small quantities of acids in combination with either ammonia soda or potash; these alkaline ulmates and humates were soluble in water, and gave an intensely brown color if the quantity of acid were at all considerable. The ammonia compounds were easily decomposed, and very frequently in waters which were not very brown, but distinctly yellow, organic acids were in combination of ammonia. These compounds were easily decomposed, sometimes by the new addition of a large quantity of water, as Dr. Frankland had shown; the brown colored humic acids being precipitated. If he were asked to name a solution which was perfectly neutral when cold, and gave off ammonia on boiling, and yet remained neutral on cooling, he. should say, try the experiment of extracting some rotten farmyard manure with water; when cold it would be perfectly neutral, but, on boiling, the ammonia would fly off, and a precipitation of brown colored humic acids would take place, and the clear liquid was neutral. He mentioned this to show that it was possible to have peaty waters very little colored, and containing more saline ammonia than many other waters; it also explained possibly the larger proportion of ammonia in peaty waters than in most other potable waters. He was glad the Chairman had confined the discussion to the question of peaty waters; and he must be allowed to utter a caution not to apply the remarks made in the paper to the self-purification of ordinary rivers, polluted with sewage. There were great differences in the facility with which various organic matters were oxidized; some oxidized so rapidly that you could not lay hold of them, whilst others were very difficult to oxidize or burn away; even with oxide of copper they could not be burned completely without the aid a a stream of oxygen or of chlorate of potash or some similar powerful oxidizing agent. The question of self-purification of ordinary river waters had been care-

fully avoided in the paper. It was unfortunate that Dr. Tidy had used an illustration which was not strictly accurate when he referred to the self-purification of peaty waters as an act of oxidation; whereas he himself fully agreed with Professor Hartley that the change was mainly due to the intervention of mineral matters. On the other hand it was also unfortunate that experiments on oxidation should have been with a substance, the very last that he should have chosen, if he wished to ascertain whether objectionable matter in river water was removed by oxidation. Of all these objectionable matters, there was hardly anything so stable and persistent in character as peat. It would be only going one step further to shake up lignite, or even powdered carbon or coke, with air and water, and see what amount of oxidation you would get. Certainly, if he wanted to show something which did not take place, he should choose the oxidation of peat; this, however, proved nothing as to the self-purification of ordinary polluted river water.

Professor CORFIELD concurred in the views just expressed. He should like to ask Professor Hartley if it had occurred to him that the large amount of evaporation which must take place in the summer, as the water went over the falls, might not account for the concentration of the water to some extent; and whether he did not prove too much in showing that the organic nitrogen and carbon were not reduced, and even in some cases increased; whether such apparent increase might not be due, especially in the case of chlorine, of the actual concentration of the water by evaporation. This took place in water flowing over land, especially in the summer, as sewage water sometimes passed off at the other side of a field more concentrated, as regards several constituents, than when it came on.

Mr. HOMERSHAM said it was quite correct, as stated by Mr. Hartley, that peaty water, at a warm temperature, held in solution a greater quantity of peat than when cold. In 1847, he had occasion to have some very exact experiments made on this subject, by the late Professor Ronalds and the late Mr. George Newport, F.R.S. The results were as follows:—Water from 57° to 65°, held peat in solution to the extent of about five grains per gallon; when the temperature was increased from 55° to 67° it held something like $7\frac{3}{4}$ grains; and when it was increased to 84°, there was as much as 14 grains. This was ascertained with water and peat from the Manchester water-works drainage ground, the peat, in many places, being 16 feet thick. Professor Hartley had treated the subject purely from a chemical point of view, but those who had practical experience of peaty water, and other water collected in large reservoirs, knew that something else had to be taken into consideration. There was the biological aspect of the question. You could not take a slow stream, running even a short distance in warm seasons, without getting a variety of living organisms in the water, first vegetable and then animal; these tended to work a marked effect on the color of the water. If you took water such as that from Loch Katrine, or the large impounding reservoirs that supplied Manchester, you would find twenty or thirty different sorts of organisms, vegetable, animal, and diatomaceæ in a gallon. If he had been seeking a title for the paper he should have called it, on the self-pollution of river waters, for he knew of no river where the water at its source was not much purer than it was lower down. In the spring, blossoms, and in the autumn, leaves, unavoidably fell into it, besides other foul matter, and thus the water became impure the further it flowed. Even in districts where peat lay heavily on the ground, you often got springs issuing from the sides of the hills of a most pure character, perfectly free from organic, and almost free from mineral impurity, but when that water was conducted through closed pipes, and was impounded in spring water lodges for bleaching purposes, vegetable organisms soon pervaded the water. He had seen very pure water at the head of a stream in dry weather, which in a very short run was filled with large quantities of a very fine weed, so much so, that in warm weather it took a man every three or four miles to clear it out. The weed grew rapidly, and it decayed almost as rapidly. As soon as it decayed, small animal life appeared in the water preying upon it. He knew of no river or spring water exposed to the air which did not get, more or less, polluted in this way.

Mr. WARINGTON thought it would be found, in Professor Way's earliest paper on the absorptive power of soils, that clay would decolorize an aqueous solution brown in color from the presence of organic matter. He found that such solutions, when filtered through clay, or when clay was added to them in sufficient quantities, were decolorized. There was no doubt Mr. Hartley had pointed out what was the real process in nature by which these coloring matters were discharged from peaty water. Professor Detmer, who had worked a good deal on humic acid, had come to the conclusion that it was a colloid body, and this enabled one to understand better how it might be carried down in a pseudo-mechanical way by precipitants. Hardly any soil could be taken, if it contained clay, which would not be found perfectly efficient in removing the coloring matter from water contaminated with peat. But if they came to the conclusion that peat was not oxidized in water by aeration, it did not necessarily follow as a corollary, that this non-oxidation was due to the fact that peat was a vegetable substance. It had been too hastily concluded by some that vegetable nitrogenous substances did not nitrify, whereas animal nitrogenous substances did so readily. Although some vegetable substances were hard to be attacked by oxidizing agents, whether by organisms, or by the oxygen of the air, yet all were not alike, some being easily oxidized. If they wished to know what substances were capable of being destroyed in a river, they had simply to inquire whether they were capable of supporting the life of bacteria, or of affording food to plants; if so, they would, under favorable circumstances, be destroyed.

Mr. DE RANCE said he should like to offer a remark in confirmation of the views of his colleague, Mr. G. H. Kinahan. It was noticeable in the English Lake District, where the tops of the hills were covered with peat, and the water very brown, that when it came in contact with the refuse from the mines, it soon became colorless. It was worthy of note, also, that in areas where, from the nature of the strata, the water was of some hardness, from the presence of chemical constituents, it was far less peat-stained than in soft-water districts. It was only in areas like portions of Lancashire and Cornwall, where the strata were millstone or granitic, yielding soft waters, that you got those deep amber colored waters which painters were so fond of delineating. This no doubt was of some importance in a chemical point of view, but being a geologist, he was not prepared to discuss it. With regard to the differences observed in summer and winter, he might remark that the sources of these rivers were generally at a considerable elevation, where the temperature was low in winter; and, in fact, last autumn, when engaged on the geological survey of Yorkshire, he had found the frost so hard, that he could walk in safety on what, in the summer, would have been an impassable quagmire; and the snow, which was then lying on the ground, being melted by the sun, ran off comparatively pure. In summer, again, there were dry times when the peat rotted away, and a heavy thunder shower would carry down large quantities of it; but in winter, when the wet weather was more continuous most of the peaty matter would be carried down the first day or two, and afterwards the water would be comparatively pure

Mr. R. B. WHITE said indigo, annatto, and other coloring matters were precipitated from water by lime, the process being well known to practical dyers. In the Cordilleras and the Andes there were large areas of peat bogs, and the rivers descended not a few hundred, but six or seven thousand feet, before they reached the plains, and as they did not lose their color in the descent, the observations of Prof. Hartley were well borne out. But after they reached the plains and their waters mingled with other rivers containing large quantities of alluvial matter in suspension, the water became comparatively clear, and much more drinkable. The natives of those countries were quite aware that those waters which appeared a little turbid were more wholesome than the clear but dark colored waters from the hills.

Mr. HARTLEY in reply, said the amount of evaporation in one of the experiments could have been next to nothing, the air being saturated with moisture at the time. A slight effect might in some cases be produced from this cause. With regard to the biological aspect of the question,

and the disappearance of the carbon and the nitrogen in the water from this cause, it must, of course, be taken into consideration in a sluggish river, and especially in the summer time; but in the time occupied in falling 360 or 700 feet no biological action could take place, and therefore the cause of the reduction of carbon or nitrogen must be sought either in the chemical action of the air or of the material mixed with the water. He had not referred to Professor Way's papers, as he had worked at the subject entirely from his own point of view, but he was glad to find that the results agreed so well.

Mr. WARINGTON said he did not mean to intimate that Professor Way had experimented on peat, but with such brown drainage waters as occurred on a farm, colored with humic matter.

Mr. HARTLEY said the results obtained by Prof. Way seemed to confirm those obtained by Mr. Kinahan in his laboratory. With regard to the descent of peaty water over falls of 5,000 or 6,000 feet, with the results which had been mentioned, it was a most satisfactory confirmation of the view that there could be no considerable amount of oxidation taking place by the action of the air, because, if such an action took place as had been supposed with a fall of fifty feet, in the case of the South American rivers, they ought to be completely bleached, even if they were almost black before the aeration took place.

RESERVOIR CONSTRUCTION.

By MAX KRAFT.

"Wochenschrift des österreichischen Ingenieur-und Architekten Vereins. From Abstracts of the Institution of Civil Engineers.

THE reservoirs constructed to supply the motive power to the machinery employed in the mine-workings near Freiburg are reckoned among the most important of the kind in Germany. They form an upper and a lower system of reservoirs of various capacities, and are situated between the rivers Mulde and Flöka. The catch-water drains and supply channels are together over 10 German miles in length, and are constructed, for the most part, in solid masonry of an oval section, 5 feet 8 inches in width, and 2 feet in depth; some of them are even navigable.

The water thus collected is stored in eleven reservoirs, of which the most important are the Grosshartsmannsdorf, the Dörnthal, and the Dittmannsdorf.

The country being open, these reservoirs are noteworthy for the length rather than for the height of their banks. As regards capacity, the Lower Grosshartsmannsdorf is the most important, while the Dörnthal reservoir has the highest embankment.

Let L = length of embankment.
H = height of embankment.
h = height of embankment above water-level.
S = width of base of embankment.
B = width of top of embankment.
h_1 = depth of outlet below top water-level.
w = width of waste weir.
t = depth of waste weir.

Then the following table gives the chief dimensions of the most important reservoirs:

Name of Reservoir.	Capacity.	Area.	L	H.	h.	S.	B.	h_1.	w.	t.
	Cub. meters (= 220 gallons.)	Sq. met. (=10.764 sq. feet.)	Meters (=3.28 feet.)	Met.	Met.	Met.	Meters.	Met.	Met.	Met.
1. Up'er Grosshartsmannsdorf	570.790	194,890	491.0	10.43	0.737	55.7	11.4 to 13	..	3.4	1.14
2. Middle do.	320,171	122,324	629.5	9.8	0.74	53.5	15.9 " 17	9.5	1.8	1.10
3. Lower do.	1,456,116	601,926	725.4	8.3	0.89	46.6	17.0	7.4	..	..
4. Dörnthal	1,283,869	199,029	284.4	16.5	0.74	54.0	12.5	13.1	6.8	2.3
5. Dittmannsdorf	528,982	101,726	376.2	13.1	0.89	45.2	11.4	8.5	7.9	1.9

The total capacity of the eleven reservoirs that constitute this system is 5,083,324 cubic meters, equal to a daily supply of 13,926.9 cubic meters, without taking into account the fact that these reservoirs are filled 1.5 time in the course of the year.

Again, taking B and S in terms of the height of bank, and

$a+a_1$=the sum of both slopes in terms of H.

q=capacity corresponding to each unit of height of bank.

q_1=capacity correspondiug to each unit of length.

f=capacity corresponding to each square meter of the area enclosed.

The following results are obtained:

	B.	S.	$a+a_1$.	q.	q_1.	f.
				Cubic meters.		
1. Upper Grosshartsmannsdorf ..	1.09−1.2H	5.3H	4.1−4.2H	54,726	1162.4	2.92
2. Middle do.	1.53−1.7H	5.4H	3.87−3.7H	32,671	514.1	2.62
3. Lower do.	2H	5.5H	3.5H	175,074	2007.3	2.42
4. Dörnthal	0.75H	3.2H	2.5H	77,810	4514.2	6.45
5. Dittmannsdorf.....	0.87H	3.4H	2.53H	40,380	1406.1	5.20

Which furnish a ready means of comparing the relative advantages as regards the situation and construction of each reservoir. A similar table, showing the cost of each, unfortunately cannot be constructed, as the data necessary are obtainable only for those built in later years, viz., the Dörnthal and Dittmannsdorf reservoirs, which cost per cubic meter of capacity 0.354 and 0.572 of a shilling respectively, which is, considering the dimensions of the banks, exceedingly low in comparison with recent masonry dams. The embankments on the Freiburg system are similar to those in Austria, so far as the materials, clay and earth, employed in their construction are concerned, but differ in the manner in which they are disposed. In Austria the puddle is laid in the center; in Freiburg it forms the inner slope of the bank, which has this advantage, that it keeps the entire bank dry and free from water; whereas the Austrian mode of construction admits of it penetrating through one-half of the bank, which, under certain circumstances, may be attended with disastrous consequences.

The Dörnthal reservoir is an excellent type of all the others. It is very well situated in a valley running south-east and north-west, and was designed originally, in 1787, with a bank 27½ feet high; this, however, was increased to 33½ feet in 1791, in which year it was built. In 1842–44 the present bank was constructed at its present height (54 feet) on a new site. The base of the bank reposes on the *débris* overlying the gneiss, through which the clay portion of the bank is carried down 11½ feet into the solid rock, which was blasted out to a width of 56 feet; below this again, a trench, 9 feet wide and 6 feet deep, was also cut the entire length of the bank, terminating at each extremity in returns at right angles to the center line of the bank. Thus the inside portion of the bank is composed entirely of a mass of clay (56 feet) at its base, and 18½ feet at the top; its outer slope being protected from the action of the waves by squared stone pitching. The top of the bank is also covered by a 1 foot 7½ inches coating of well-rammed puddle, and on its inner edge a solid masonry breast-wall, extending its entire length, is built.

Three cast-iron out-let pipes are laid in the bank at different levels; the valves of these pipes are of the ordinary Austrian pattern, and are worked from the valve-house by square iron rods resting on rollers.

The drainage from the country in the neighborhood of the reservoirs is, for the most part, intercepted by catch-water drains, and supplied to the landowners and mill proprietors, whose claims have to be considered. The waste-weir is of solid ashlar; a series of cast-iron standards is provided, the grooves of which receive planks 6 inches wide and 3 inches thick; each of these is provided with an iron hook in the center, so that by means of a rod they can be lowered or raised at pleasure, and the top water-level of the reservoir regulated accordingly. The

waste-water course descends by two vertical steps to the level of the artificial canal provided for the overflow. The reservoir is supplied from the Friedrich Benno tunnel and the brook flowing into it; the latter has, however, to be delivered undiminished into its natural channel, for the use of those entitled to the water.

BACK PRESSURE ON VALVES;

OR THE EQUILIBRIUM LINE BY EXPERIMENT AND THEORY FOR BROAD-SEATED VALVES.

Read at the Annual Meeting of the American Society of Mechanical Engineers, November, 1882,

By S. W. ROBINSON, Prof. Mechanical Engineering Ohio State University.

THE title of this paper perhaps poorly indicates its true import. More specifically, an attempt is made to determine by experiment what fractional part of that pressure exerted upon the top surface of a valve by a fluid is required, when acting from below, to raise that valve from its seat. The valve is supposed to have a broad surface of contact with its seat, as in the case of a slide valve in the steam-chest of an engine where the flanges have a broad lap.

Every designer of steam engines with slide valves finds the question asked of himself, "What force is necessary to slide the valve on its seat?" The first showing of an answer may be, "It depends on the pressure and the coefficient of friction." Probably but little is positively known about either the *effective* pressure or the coeffcient of friction in this case. The present object is to throw light upon the question of effective pressure.

To illustrate, suppose a common D slide valve has a surface contact with its seat of $12'' \times 12''$ outside, less a $5'' \times 10''$ D cavity inside, as shown in Fig. 1, the cavity being supposed in communication with the exhaust. It seems evident that the effective pressure is not $p_1 \times 12 \times 12 = 144 p_1$, where p_1 is the pressure in lbs. per square inch, because why may it not be nearly the $p_1 \times 5 \times 10 = 50 p_1$ as due to the cavity itself? The most thoughtful, probably, will assume some area between the inside and outside, in the neighborhood of that inclosed by the dotted line, by which to multiply p_1 for the effective pressure. This line I hereafter in this paper call the "*equilibrium line*," and the area inclosed within it the *equilibrium area*," because the pressure p_1, multiplied by it, equals a force which, acting from below, will just lift the valve from its seat.

When the equilibrium area falls between that of the cavity and of the outside of the valve, it appears that the fluid, pressing on the top of the valve creeps under the edges to some extent, and that the film of fluid under the valve near the cavity relieves itself by escape into the cavity. It would seem from this reasoning—required for explaining the equilibrium line—that for true surfaces, a film of almost infinitesimal thinness is constantly creeping along between the "contact" surfaces toward the cavity, and that on this route the pressure of the creeping fluid is continually falling, starting with nearly that of the higher pressure side of the valve, and ending at the cavity with the pressure of the latter.

When the whole lifting pressure exerted by the creeping fluid between surfaces is known, the equilibrium line can be located. The theoretical determination of this lifting action is the same as that for finding the force tending to throw a packing ring from its seat. Formulas for the latter are given in this MAGAZINE, Vol. XXIV., p. 441, and following, in my paper on *A Rational System of Piston Packing*.* According to that paper it appears that a rectangular valve like Fig. 1, and for an incom-

* I take occasion to here correct an oversight in this paper as published in this Magazine, but corrected as appearing in the proceedings of the Am. Soc. Mech. Engineers, viz.: In lower line of 1st column of page 445, after "Hence," add "hereafter calling P the absolute pressure, and p_2 the back pressure;" and in equation (9) read the first member "$P - p_2$." Also page 448, in the "Practical Table for Single Ring Packing," regard the figures "4," "6," "8," "10," as raised up one place higher; and below 10 add "12." Also opposite 4, thus raised, put at the head of the 1st column to the right, ".0431;" and at head of last column ".0451."

pressible fluid, the pressure falls on a straight-line declivity from p to p_2, p being the pressure just at entry, and p_2 that within the cavity. The fall from p_1 to p is due to the acceleration for entry into the space. The latter, it is shown in the paper cited, will be a small fraction of the whole, but will be neglected here.

In Fig. 2 let the shaded area stand for a section through a D slide valve, like Fig. 1, the square outlines being best for the present purpose.

If p_1 be the absolute pressure on the top of the valve, and p_2 the absolute back pressure in the cavity, then, according to to what is said above, EF will represent the line of falling pressure from p_1 to p_2. Also it is evident that if the area IJKL stands for the total absolute downward pressure upon the top of the valve, then the area AEFC will represent the total absolute lifting pressure at one side of the valve due to the creeping fluid along AC, and tending to raise the valve from its seat. Now draw EG parallel to AC, and make the area AEGH = AEFC. Then the lifting pressure of the creeping fluid neutralizes so much of the top pressure IJKL as lies directly over the length AH. Hence H is the position of the equilibrium line. Likewise we may find the point on BD.

From the figure it appears that AH will equal a half AC only when $p_2 = 0$.

As an example, suppose $p_1 = 60$ and $p_2 = 15$. Then the mean pressure on AC is

$$P = \frac{p_1 + p_2}{2} = 37.5$$

and

$$P \times AC = p_1 AH,$$

or

$$AH = AC\frac{P}{p_1} = AC\frac{37.5}{60} = AC.625.$$

Hence the equilibrium line is nearer C than A. More specifically, it appears that 62½ per cent. of the pressure upon that part of the top of the valve which lies above AC and BD is counteracted by the lifting action of the creeping fluid.

This supposes the fluid incompressible like water (nearly so). Hence the above calculation is suited to the case of a valve working in water. For quite a range of pressures under this case we may quote from the paper on piston packing referred to:

$\frac{p_1}{p_2}$	= 2	$\frac{P}{p_2}$	= 1.5	$\frac{P}{p_1}$	= .750	
"	4	"	2.5		.625	
"	6	"	3.5		.583	.....A.
"	8	"	4.5		.562	
"	10	"	5.5		.550	

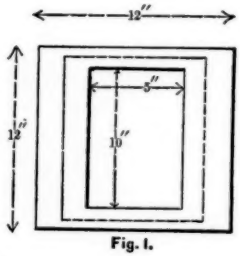


Fig. 1.

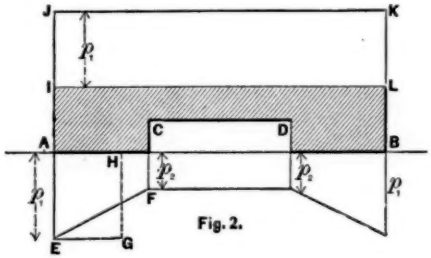


Fig. 2.

where p_1 is the absolute pressure upon the top of the valve, p_2 the absolute back pressure, and P the mean pressure due to creeping fluid. Then

$$AH = AC\frac{P}{p_1} \quad . \quad . \quad . \quad (1).$$

thus locating the equilibrium line.

If the fluid is elastic, like air, and the flow in the space AC is adiabatic, we have from the same source:

$\frac{p_1}{p_2}$	= 4	$\frac{P}{p_2}$	= 2.72	$\frac{P}{p_1}$	= .680	
	6		3.94		.657	
	8		5.18		.647	...B.
	10		6.42		.642	

And for the isothermal flow of elastic fluids:

$\frac{p_1}{p_2}$	= 4	$\frac{P}{p_2}$	= 2.80	$\frac{P}{p_1}$	= .700	
	6		4.09		.682	
	8		5.41		.676	...C.
	10		6.73		.673	

These tables differ but little, and values may be selected to suit any case in hand,

including even a condensable fluid like steam, because whether steam, or water due to condensation, fills the space AC, the case will fall within the tables above. If an exception is to be made on the supposition that, with steam, the percentage of water will increase on the route from A to C, it will be to the effect that the lifting force is greater than the calculation would give, and hence on the safe side.

To illustrate further the use of the above tables and formula, suppose a D slide valve in a steam or air chest is 10 inches over all, and with a D cavity of 4 inches. Each flange will then be 3 inches if equal. Further, take $p_1=90$, $p_2=15$. Then

$\frac{p_1}{p_2}=6$ AH=AC .682, or AC .583

=2.05 inches or 1.75 ins.

if the last table be selected, or the first respectively.

Hence, doubling the values, we get for both flanges outside the equilibrium line: 4.1 or 3.5 inches respectively, or within the equilibrium line 5.9 or 6.5 inches respectively.

Hence the effective pressure of the valve upon its seat is as though the last-named widths of equilibrium area only were subject to the pressure p_1.

THE EQUILIBRIUM LINE BY EXPERIMENT.—In the Spring of 1881 Student J. H. McEwen subjected this question of back pressure to experiment, in my mechanical laboratory, as a practical problem in course. The appliance explained below was made by him for the purpose. In experimenting the data were procured partly by Mr. McEwen and partly by Mr. C. F. Marvin. The results obtained from reducing the data to the valve are given in the plate.

The appliance employed is shown in section in Fig. 3, in which A represents the valve, B its seat, D the bed-plate of the containing chest, C an inverted hollow piece, which, when bolted to D, together constituted the chest, G a nozzle for admitting the fluid pressure, H a cock for draining the chest, E a lever, which, by means of a strut reaching up through the seat, will lift the valve A, and F the scale pan for loading the lever.

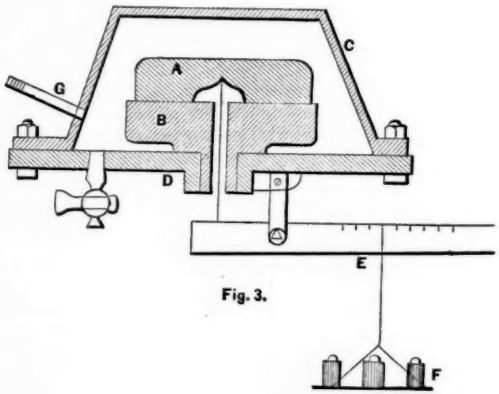


Fig. 3.

The valve and seat are circular in plan, and not rectangular. They were carefully fitted together by facing truly in a lathe, and then by grinding with emery. The grinding was done partly in the lathe and partly by hand with cross motion. It is perhaps to be regretted that this fitting was not done by scraping, but as they were, the surfaces possessed the properties of surface plates in high degree. Placing A upon B with clean joints, the former would float upon the latter for five or ten seconds, lubricated by air. When settled, A would lift B and support it for a considerable time. The surfaces were mirror-like for smoothness throughout. Though the job was well done for grinding with emery flour, yet it is believed that a somewhat more satisfactory contact could have been ob-

tained by the scraper. The outside diameter of the valve A is six inches, and its cavity two and one-tenth inches. The seat B was about three-fourths of an inch larger than A outside, with a central aperture of about one inch. This excess of surface on B was intended for insuring contact on the entire lower surface of A, even should it get eccentric with B. The seat B was mostly free from the bed-plate, so as to relive it from the strains and flexure of the latter. A neck projecting down from B was pressed into the bed D, with a free space between B and D, thus supporting B on a short leg. The strut from the lever E nearly filled the central hole in D, and extended to the bottom of the cavity in A. This cavity in A was made conical at the center, so that the strut would always carry A concentric, and poised as it was raised. The thickness of A and B was about two inches. The lever was borne on knife-edges.

Steam is the only fluid experimented with. It was admitted at the nozzle G; condensed water was let out at H.

In the experiments weights were put on till the valve was raised, or, as in some cases, the weights were shifted along the graduated lever. The pressure of steam in the chamber upon the top surface of the valve was varied from 10 to 84 lbs. per square inch by gauge, or from 25 to 99 lbs. per square inch absolute. The results reduced to the valve are given in the plate where the circle dots were obtained by one series of observations and the cross points by another, the two series being several weeks apart in points of time. The vertical scale at the left is for the steam gauge pressure in the chamber. The horizontal scale gives the number of lbs. pressure per square inch of the cavity area which was necessary to lift the valve. The figures given in this scale were reduced to the valve from the experimental data read off from the weighted lever, the weight of the lever, scale-pan, strut and valve being taken into account, but not the atmospheric pressure to which the cavity under the valve was exposed. Hence the figures in both scales are apparent pressures per square inch—one for the entire area of the six-inch circle of the valve, and the other for the area of the circle of the cavity, two and one-tenth inches in diameter.

The curve running among the dots is supposed to be drawn where the sum of the squares of all the distances from the line to the dots is the least, that is to say, that line is the probability line for all the observations, according to the theory of "*least squares*." It is assumed, however, that the line should run through the origin. From this line the figures in the first two columns of the following table were read off:

TABLE D.—FOR A CIRCULAR VALVE 6 INCHES IN OUTSIDE DIAMETER, WITH A CIRCULAR CAVITY 2.1″ DIAM. EXPOSED TO THE ATMOSPHERE.

Appt. pressure lbs. per sq. in. on valve 6″ diam. $p_1 - p_2$	Appt. pressure lbs. per sq. in. on cavity of valve 2.1″ diam. p'	Equilib. area such that $a'(p_1 - p_2)$ will just raise valve: sq. inches. a'	Diam. of equilib. area inches. d.
5	8.	5.6	2.6
10	17.	5.8	2.7
15	26.	6.0	2.8
20	36.	6.2	2.8
25	46.	6.4	2.9
30	57.	6.6	2.9
35	69.	6.8	2.9
40	81.	7.0	3.0
45	95.	7.3	3.0
50	112.	7.8	3.1
55	129.	8.2	3.2
60	150.	8.7	3.3
65	172.	9.2	3.4
70	198.	9.8	3.5
75	230.	10.5	3.7

A better notion of the quantities given in the table and plate perhaps can be obtained from Fig. 4, giving a section of the valve and its seat. Within the valve outline are drawn arching circles springing from the seat. These define the greatest and least equilibrium lines, which here are circles to the diameters 3.7″ and 2.6″ respectively, between which all the figures in the last column of the table fall. The interpretation of Fig. 4 is this: For 75 lbs. apparent pressure per square inch upon the top of the valve, the equilibrium circle is 3.7″ in diameter. That is, if the valve be taken from the chamber, and its seating face be turned off or cut away, with the exception of a sharp circular line to the diameter of 3.7″, so that when the valve is returned to its seat, this circle is the only bearing of the valve upon its seat; then the same lifting force will be re-

quired to raise the valve from its seat as before, for this stated pressure of 75 lbs. Again when the stated apparent pressure is 5 lbs. per square inch upon the top of the valve, then the same lifting force will raise it as when its face is cut away to a ring-line bearing of 2.6″ in diameter. In each case the atmospheric pressure is in full upon the cavity.

The relation between the pressures and areas may be expressed thus:

Let p_1=the absolute pressure per square inch within chamber.

p_2=ordinary back pressure upon the cavity.

formula, observing that a=3.46 square inches, give a'=7.8 square inches, as found in the third column, for the equilibrium area. But this formula is only applicable to this particular valve.

It seems desirable that the theoretical formulas given above for rectangular valves be brought to the test of verification or disproof, by this series of experiments. To do so it will be necessary to deduce a general formula for circular valves in the same way as was that for rectangular valves, and then compare that theoretical formula for circular valves with the experimental valves of the table. If the theoretical results agree with the

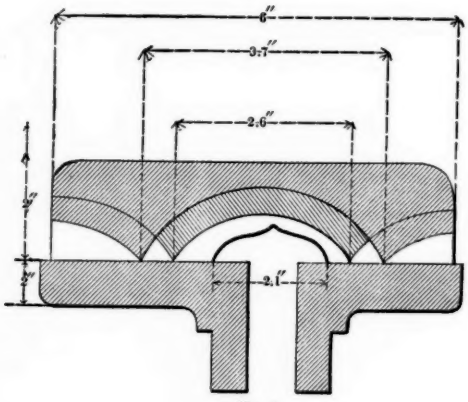


Fig. 4.

p'=lifting force per square inch of cavity, approximate.

P=mean absolute lifting pressure per square inch, exerted by the creeping fluid between A and B, Fig. 3.

A=area of valve, here 6″ diameter.

a= " " " cavity, here 2.1″ diameter.

a'=equilibrium area.

Then,

$$p_1A=p_1(A-a')+p'a+p_2a'$$

or

$$a'(p_1-p_2)=p'a$$

$$a'=a\frac{p'}{p_1-p_2} \quad . \quad . \quad . \quad (2).$$

As an example, take the pressure p_1-p_2=50 lbs. for the first column of table. Opposite this, in the second column, we find p'=112 lbs. These, in the above

experimental ones, then not only the theoretical formula for circular valves is verified, but that for rectangular valves, also.

From my paper on *A Rational System of Piston Packing*, above referred to, p. 446 of this Magazine, we may quote,

$$p-p_2=\frac{\delta f s l}{a}\frac{v^2}{2g} \quad . \quad . \quad . \quad (3).$$

as expressing the fall of pressure p to p_2 in a space conducting a fluid of invariable density d, with the velocity v, where l is the length of the space, a its section, s the perimeter of the cross section, and f the coefficient of friction of the fluid upon the surfaces.

If the space be very thin, with a thickness t, then, for a unit's width we have s =2 and $a=t$, so that

$$\frac{s}{a}=\frac{2}{t} \quad . \quad . \quad . \quad . \quad (4).$$

Then for a very short length we may put dx for l, and dp for $p-p_2$, giving

$$dp=\frac{2\delta f}{t}\frac{v^2}{2g}dx \quad . \quad . \quad . \quad . \quad (5).$$

Applying this to the part $a\,b\,c\,d$ of the space A B C D, where the thickness is constant, the velocity of the incompressible fluid will vary as it moves from A C to B D. This velocity will, in fact, be inversely proportional to the distance from O. So that, if v_2 = the velocity at B D, we have, if r = B O, A B = b, A O = R, and a O = x,

$$\frac{v}{v_2}=\frac{r}{x} \quad . \quad . \quad . \quad . \quad (6).$$

Then, replacing v by its value,

$$dp=\frac{2\delta f}{t}\frac{v_2^2}{2g}\frac{r^2}{x^2}dx \quad . \quad . \quad (7).$$

Integrating this between the limits AO and BO; or x and r, and p and p_2, we have

$$p-p_2=\frac{2\delta f}{t}\frac{v_2^2}{2g}r^2\left(\frac{1}{r}-\frac{1}{x}\right) \quad . \quad . \quad (8).$$

Here p is the pressure in the space A B C D at the line $a\,d$, situated at the distance x from O. This line is circular, and may be considered as extending to the entire circle about O. Then the absolute pressure upon a circular strip of width dx, and radius x, is

$$p2\pi x dx,$$

and for the entire under surface of the valve A, the total absolute lifting pressure is

$$P\pi(R^2-r^2)$$

a quantity which is equal to the integral of the last expression above. Writing this equation, and at the same time introducing p form equation (8) above, we obtain

$$P\pi(R^2-r^2)=\int_r^R\frac{2\delta f}{t}\frac{v_2^2}{2g}r^2\left(\frac{1}{r}-\frac{1}{x}\right)2\pi x dx + \int_r^R p_2 2\pi x\,dx$$

$$=\frac{2\delta f}{t}\frac{v_2^2}{2g}\pi r^2\left\{\frac{R^2-r^2}{r}-2(R-r)\right\} + p_2\pi(R^2-r^2)$$

Whence,

$$P=\frac{2\delta f}{t}\frac{v_2^2}{2g}r^2\left\{\frac{1}{r}-\frac{2}{R+r}\right\}+p_2$$

$$=(p_1-p_2)\frac{R}{R+r}+p_2 \quad . \quad . \quad . \quad (9).$$

since, from equation (8), for pressure, we have

$$(p_1-p_2)\frac{Rr}{R-r}=\frac{2\delta f}{t}\frac{v_2^2}{2g}r^2$$

Dividing by p_2, we obtain

$$\frac{P}{p_2}=\left(\frac{p_1}{p_2}-1\right)\frac{R}{R+r}+1 \quad . \quad . \quad (10).$$

in which $R=b+r$. See Fig. 5.

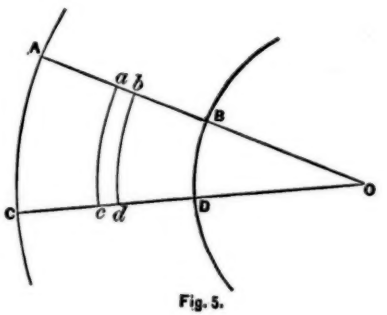


Fig. 5.

Or, if the relation of the mean absolute lifting pressure P, due to the creeping fluid, is desired in its relation to p_1, the last equation is readily modified to that end, viz.:

$$\frac{P}{p_1}=\left(1-\frac{p_2}{p_1}\right)\frac{R}{R+r}+\frac{p_2}{p_1} \quad . \quad . \quad (11).$$

These equations are for an inelastic fluid like water. For elastic fluids, even for the isothermal condition, the analyses are too complex to be introduced here for the circular valve. But for rectangular valves of the form of the ordinary slide valve, the solution is already given above in connection with Figs. 1 and 2. See tables B and C, and eq. (1).

But in comparing tables A, B and C, we observe that the mean lifting absolute pressure P, divided by the absolute pressure p_1 upon the top of the valve, gives a ratio which is nearly the same in the three tables, for any one pressure p_1. Or more briefly, it seems to make but

little difference whether the fluid, pressing upon the valve and creeping along under it, is compressible like air or steam, or whether it is more nearly incompressible, like water. Hence, according to equation (1), it appears that the equilibrium line is nearly at the same position, whatever the fluid, though for high pressures the differences in position are greatest. This fact enables us to make use of equation (11), and the theoretical position of the equilibrium line determined thereby, for incompressible fluids in comparing our theoretical results with the results of experiment in table D.

The comparison thus made will enable us to judge of the value of tables A, B, and C, for use in calculating the position of the equilibrium line in ordinary cases of the slide valve.

For our circular valve it is plain that equation (1) will not answer. But regarding Fig. 2 as a section of a circular valve, such as shown in Fig. 3, then to find what part of the top surface of valve under the pressure p_1 will be balanced by the P upon the surface A C, we have

$$P(\pi R^2 - \pi r^2) = p_1(\pi R^2 - \pi x^2),$$

the first member being the total lifting pressure due to the creeping fluid between the valve and seat, and the second member being the top pressure p_1 multiplied by such a part of the top surface taken uniformly around the outer edge of valve as shall just counteract that lifting pressure.

In the last equation, x is the radius of the circle of the equilibrium line. Hence, the diameter of that circle is twice the value of x from that equation, or,

$$d = 2x = 2R\sqrt{1 - \frac{P}{p_1}\left(1 - \frac{r^2}{R^2}\right)} \quad . (12).$$

Here d stands for the same as given in the last column of the table D. Combining (12) and (11), we obtain

$$d = 2r\sqrt{\frac{R}{r} - \frac{p_2}{p_1}\left(\frac{R}{r} - 1\right)} \quad . \quad (13).$$

for the diameter of the equilibrium circle for circular valves, as found by theory.

Now, values of d calculated by (13) should agree with the experimental values of the last column of table D, except for the differences due to the fact that the theoretical values are for inelastic fluids, while the experimental values were obtained by experiments with steam, differences which have already been shown to be small.

A comparison of the theoretical values from (13) with the experimental values of the last column of table D will therefore be of interest. This comparison is given in

TABLE E.—COMPARING THEORETICAL AND EXPERIMENTAL DIAMETERS OF THE EQUILIBRIUM LINE FOR R = 3″ AND r = 1 05″.

Appt. pressure per sq. in. on valve. $p_1 - p_2$	Ratio of pressures. $\frac{p_1}{p_2}$	Lifting and seating pressures. $\frac{P}{p_2}$	Diam. of experimental. $d.$	Equilib. area, calculated $d.$
5	1.33	.936	2.6	2.53
10	1.67	.897	2.7	2.85
15	2.	.871	2.8	2.92
20	2.33	.852	2.8	3.02
25	2.67	.838	2.9	3.09
30	3.	.827	2.9	3.14
35	3.33	.819	2.9	3.19
40	3.67	.812	3.0	3.22
45	4.	.806	3.0	3.25
50	4.33	.801	3.1	3.27
55	4.67	.797	3.2	3.29
60	5.	.793	3.3	3.31
65	5.33	.790	3.4	3.33
70	5.67	.787	3.5	3.34
75	6.	.785	3.7	3.35
∞	∞	.741	—	3.56

Probably 10 to 15 per cent. lower for elastic fluids.

Though the last two columns are, for practical purposes, identical, yet it is to be remembered that perhaps a considerable difference is due to the fact of elastic and inelastic fluids as above mentioned.

To obtain some insight to the differences of calculated values for elastic and inelastic fluids, compare Table A for inelastic fluids with Table C for elastic fluids, flowing isothermally; C being chosen in preference to B, because the differences are greater, but ranging here, however, mostly within 15 per cent. for values of $P \div p_1$. Though the law of velocity of the creeping fluid in circular valves differs from that in rectangular ones, giving cause for a difference in $P \div p_1$ for elastic and inelastic fluids, which will not be exactly the same for rectangular as for circular valves; yet it is probable that these ratios of pressures will not be very far removed from each other. Assuming these differences iden-

tical for all forms of valve, and allowing the 15 per cent. range above mentioned, then, putting in eq. (12) valves for $P+p_1$ differing by 15 per cent. from those in Table E, we find that the corresponding calculated diameters of equilibrium areas are less by about 20 per cent. than those given in the last column of Table E. This throws the calculated values all to the other side of, or makes them smaller than, the experimental diameters of the equilibrium areas of the table, showing that for an elastic fluid these experimental diameters are high by about 10 per cent. This may perhaps be accounted for by the fact that during the experiments, when the chest was frequently opened to examine the valve, the seating surfaces were found to have upon them slight traces of a fine gray deposit, and this appeared to be most dense at and near the outer edge of the seating surfaces. The tendency of this would evidently be to keep the valve slightly elevated, and to hinder the free entrance of the creeping fluid into the space under the valve, and to favor its passage and escape after entrance, thus causing a greater effective pressure of valve upon seat and enlarging the experimental equilibrium areas to the figures observed, and to values higher than those calculated on the supposition of a purely elastic fluid, flowing isothermally.

From these considerations it appears that the theoretical deductions are to be relied upon, and that the formulas resulting may be adopted in practice without hesitation. Formulas for elastic fluids and circular valves have not been worked out, but sufficient effort for them has been made to ascertain that such formulas would be neither elegant nor convenient for use. But fortunately, such formulas are mostly needed for rectangular valves with wide flanges on only two sides, as in slide valves for steam engines, for which case tables A, B and C will serve for all fluids and all requirements.

Practical formulas for calculating the equilibrium areas of ordinary rectangular slide valves of steam engines.

Let Fig. 6 represent a longitudinal section through one end of a double slide valve, with L the entire length over flanges, l the width of cavity, and q the width of the equilibrium area.

It is evident that l must include cavities in the seat as well as in the valve, and perhaps bridges between The side bearings of the valve, at the top and bottom of the cavity, are regarded as so narrow that they may be neglected.

Then the upward pressure due to P

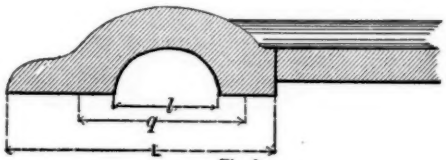


Fig. 6.

against the entire under surface of the valve is

$$P(L-l),$$

and the part of the pressure upon the top of the valve which this equilibriates, is

$$p_1(L-q),$$

which quantities are equal to each other. Equating and solving for q, we obtain

$$q=L\left\{1-\frac{P}{p_1}\left(1-\frac{l}{L}\right)\right\} \quad . \quad . \quad (14).$$

as the breadth outside of which the pressure p_1 is neutralized, and by which we are to multiply p_1 to obtain the effective pressure (absolute) upon the valve.

The total *effective* pressure of the entire half valve of Fig. 5 against its seat is therefore

$$Bq(p_1-p_2)=B(p_1-p_2)L\left\{1-\frac{P}{p_1}\left(1-\frac{l}{L}\right)\right\} \quad . \quad . \quad . \quad . \quad (15).$$

where B is the entire breadth of the valve.

As an example in application of (15), suppose a half valve be L=10″, B=12′

$l=4''$, $p_1=90$ lbs. per sq. in. absolute, and $p_2=15$ lbs. per sq. in., or one atmosphere. Then Table A for rectangular valves gives $P \div p_1 = .583$, and the total effective pressure is, by (15),

$$12.\times 75.\times 10\times [1-.583(1-.4)] = 5850 \text{ lbs.} \quad . \quad . \quad . \quad (16).$$

while the total pressure upon the entire half valve is

$$12\times 75\times 10 = 9000 \text{ lbs.},$$

which latter, it appears, will never be exerted for pressing the seating surfaces together.

The resistance to sliding for the half valve will be

$$5850f$$

where f is the coefficient of friction, the value of which no attempt is made to elucidate in this paper.

The result (16), would evidently have been the same had the cavity been placed at the middle in Fig. 6, as in ordinary slide valves.

For circular valves find d from Eq. (13), and Table E. Then the total effective pressure of valve against its seating surfaces is

$$\frac{\pi d^2}{4}(p_1-p_2) \quad . \quad . \quad (17).$$

Conical valve seats, such as for wing valves of pumps, may be treated as circular surfaces where the slant height of the cone is R, &c., and d in Eq. 13, may be found. The cone is then to be developed, and such part of $\frac{1}{4}\pi d^2$ used as the developed cone is of 360°.

Where valves have an extraneous pressure applied, as in case of springs to assist in holding the valve upon its seat, it is evident that this pressure is to be added to that above obtained for effective pressure. Such spring pressure will in effect figure the same as the presure on top of the valve over the cavity.

It may be remarked that the ratio of pressures $P \div p_1$ is for small lifts of valve independent of the thickness of the space between valve and seat. From this fact we are to conclude that the same force which starts the valve upward from its seat in testing it, is to be continued until the space becomes so thick, that the accelerative force required to give the escaping fluid its motion, becomes so great as to absorb an appreciable portion of the total head causing flow. The acceleration to be accounted for in this problem is that at entry to the space, and also along the space to the exit. The accelerative force along the space between valve and seat would be *nil* for rectangular valves and inelastic fluids, but not for others. These points are important to a rational theory of perfect "pop" safety valves.

It is advisable to here observe that the experimental results given in this paper serve not only to confirm the theory of the equilibrium line, &c., for valves, but that the theory of packing rings, as given in the paper on *A Rational System of Piston Packing*, above cited, based upon the same principles, also receives confirmation in the same degree.

INDUCED CURRENTS IN A MAGNETIC FIELD.

By LIEUT. J. B. MURDOCK, U. S. Navy.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE great development of modern electricity can be directly traced to the discovery by Faraday in 1831, of the principle of electro-magnetic induction. From his discovery, that the movement of a conductor near a fixed magnet, or a closed circuit through which a current was flowing, caused a current to flow though the conductor moved, has arisen the dynamo machine with all its applications. The principles, therefore, underlying the observed facts are of great commercial importance. Since the days of Faraday they have been carefully studied, and from the results of these investigations many useful, practical relations can be drawn.

In order to obtain the mathematical relations in a practical form several definitions and conventions have to be established, and it will be advisable to consider some of these. The region,

whether near a magnet or not, throughout which magnetic effects are produced is called a *magnetic field.* Thus every current causes a magnetic field, as is evident from the deflection of a magnet near it. A unit magnetic pole is a magnet pole of such a strength that when placed one centimeter from an equal and similar pole, it would repel it with the force of one dyne. A magnetic field is of unit intensity when a unit pole placed in it is acted upon with a force of one degree. By comparing the last two definitions, unit pole evidently produces unit field at unit distance. The same intensity would evidently exist at a greater distance from a more powerful pole, as at two centimeters from a pole of strength four, or as three centimeters from a pole of strength nine. When a numerical value is given to the strength of a field, the evident meaning is that a unit pole would be acted on by a force of that number of dynes. As an intermediate step in comparing the magnetic effects of magnet poles and voltaic currents, the conception of a *magnetic shell* is useful. This is supposed to be a thin disc of any shape, having its opposite side of opposite magnetic polarity.

In order to investigate the action in a magnetic field, it is sufficient in any case to know the direction and intensity of the force acting on unit pole at all points in the field, and any scheme delineating graphically, or otherwise, the force and direction of its application at all points, will be a true representation of the field. One of the earliest and best methods is that due to Faraday, of sprinkling iron filings on a sheet of paper over a magnet. The experiment is common, and the curved lines in which the filings arrange themselves between the poles are well known. Made in this way, the resulting curves are not strictly accurate, as they are all in one plane, while the field extends in all directions around the magnet, but they illustrate clearly how a field may be considered to be mapped out, and are of assistance in comprehending other suppositions. These curves evidently show the direction of the magnetic forces, and are therefore called lines of magnetic induction, or more commonly *lines of force.* The force on a unit pole is the greater the nearer it is to the magnet, and in this experiment the lines of force are more numerous near the magnets, leading immediately to the conception that the closeness of the lines to each other may be a measure of the force, and consequently of the intensity of the field at any point. Maxwell has shown that if in any part of the field the number of lines of force passing through unit area is proportional to the magnetic force at that point, the number passing through unit area in any other part will correctly represent the magnetic force there. This conception leads to a new definition of inten-ity of field, which is that the intensity of field at any point is the number of lines of force passing through a square centimeter perpendicular to the lines at that point. In a unit field, therefore, one line of force passes through each square centimeter of a plane perpendicular to the lines of force. If the lines of force are parallel as are those of the earth's magnetism in any small area, the field is said to be uniform.

A line of force being the direction in which the magnetic forces act, is shown at any point by the direction taken by a suspended magnet. It is impossible to imagine two lines of force crossing at a point in space, as two conflicting forces at that point would unite to form a resultant force, and the direction of this would be the line of force passing through the points. Every magnet consists of two poles, which in any field are acted upon by forces in different directions, but being inseparable the resultant motion of the magnet is due to the difference of the forces. It is easy to see, however, from any observed motion how a single north pole would move if it were perfectly free, and this gives the positive direction of the lines of force, as that in which a free north pole would move. This, in the field, is of course from north to south polarity. It is of the utmost importance to bear this convention clearly in mind. As the lines of force indicate the direction of the force, there can be no force exerted in a direction at right angles to them. Surfaces everywhere normal to the lines of force are called equipotential surfaces, and the relation between the lines and the surfaces enables one to be derived from the other.

It may be well to investigate one or two special cases illustrating the conventions already adopted. In a uniform

field, as the lines of force are parallel, the equipotential surfaces are planes. The direction of the lines is in the earth's field indicated by the dipping needle, and the positive direction is downward in the northern hemisphere. In the case of a free pole the lines of force are radial from the point and the equipotential surfaces are spheres. If the pole is of unit strength there must be one line of force through every square centimeter of a sphere of unit radius, making 4π lines of force in all. The number for a pole of strength m would similarly be $4\pi m$, and the intensity of the field at any point would be shown by the number of these lines passing through unit area of an equipotential surface containing the point. The positive direction of the lines of force is from the pole if it is north, towards it if it is south.

If a magnet pole be moved in a magnetic field work must be done. This work may consist of work done in moving the pole against a repelling force, or in preventing its movement by an attractive force. As these are directly different the former is called positive and the latter negative work. If now, after positive work has been done on the pole, as for instance in moving a north pole towards another, it be left to itself it will tend to move away. It has evidently acquired potential energy, and as this is due to the work previously done on it, this work becomes a measure of the potential energy gained. The term *potential* is used to express this relation, and is measured at any point by the potential energy possessed by a unit pole at that point. The definition commonly given for magnetic potential is that it is meas ured by the work required to move a unit pole from an infinite distance to that point. The work measured in ergs is numerically equal to the potential measured in units of potential. If a pole of strength two were brought up from infinity, twice the work would have to be done on it, but the potential at the point would be the same as at first. The potential is therefore equal to

$$\frac{\text{Work done in moving pole.}}{\text{Strength of pole.}}$$

Work and potential are evidently different, but are frequently confused. In every magnetic field there is a distribution of magnetic potential, the potential at a point near the magnet pole being greater than at a point more remote, and greater as the strength of the pole producing the field is greater. The potential at a point due to a pole of strength m can be proved to be $\frac{m}{r}$, r being the distance from m. All points equally distant would therefore be at the same potential, or the equipotential surfaces would in this case be spheres as already stated.

As potential is measured by work done, there can be no work done in a surface normal to the direction of the forces in the field, and consequently the equipotential surfaces would in all cases be perpendicular to the lines of force, as in any other direction some component of the force would act and work would be done.

Gauss showed that the potential due to a magnetic shell at any point would be ωi where ω is the solid angle subtended at the point by the shell, and i is the strength of the shell. At all points on an equipotential surface the value of the solid angle must be the same. The measurement of the angle becomes extremely difficult at points situated obliquely to the axis normal to the shell at its center, but it can be easily seen that as the point becomes oblique, it must come nearer to the shell. In the plane of the shell itself the solid angle, and consequently the potential are everywhere zero. The general shape of the surfaces is therefore that of deep bowls, concave towards the shell, and most remote from it on its axis. The lines of force start normal to the shell on its north side, and curve round, cutting successive equipotential surfaces at right angles to the south face, into which they fall perpendicularly.

If a pole of strength m be brought up to a point near a shell, the amount of work done will, as already stated, be m times that done in bringing a unit pole up to the same point, or m times the potential due to the shell at that point. The expression $\pm m\omega i$ is therefore a measure of the work done, being positive if it required work to be done to move the pole, negative if work had to be done to prevent its movement still farther. As we have seen the number of lines of force of a pole of

strength m is $4\pi m$. This number radiate through a solid angle 4π, which is the solid angle at the center of a sphere. Through any solid angle ω, the number of lines would therefore be $m\omega$, and representing this number, cutting the shell by N_1, the work done is expressed by $N_1 i$. Similarly, if the pole had been brought to a point at which the potential due to the shell was $\omega_1 i$, the work done would have been $m\omega_1 i$, or $N_2 i$. Potential being measured by work done, any change of potential can be obtained only from work. If, therefore, the pole be moved from one point to the other, the work done on unit pole is measured by the differences of the potentials, or,

$$v_1 - v_2 = \pm i(\omega - \omega_1)$$

If the pole m is moved, the work done is

$$m(v_1 - v_2) = \pm i(N_1 - N_2)$$

Any relation traced for a magnet shell is capable of application to a closed voltaic circuit. The lines of force of a current encircle it concentrically, as is shown by the tendency of a magnet pole to move at right-angles to the direction of the wire. If a wire carrying a current be bent into a closed curve, the lines of force in passing round the wire would combine their effects on a magnet pole placed near, so that the resultant line of force would be normal to the plane of the circuit; or, in other words, the magnetic effect of a closed circuit is the same as that of a magnetic shell. By adopting a particular ratio of units the magnetic effect of any voltaic circuit may be made equal to that of a magnetic shell of a given strength whose edges coincide in position with the circuit. The electromagnetic unit of current fulfills this relation, being that current, unit length of which acts with a force of one dyne on unit pole at unit distance. This unit of current, therefore, causes unit field at unit distance, as by definition does also a shell of unit strength. By measuring our current, therefore, in these units, the relations traced for the field due to a shell can be applied to that produced by a voltaic circuit.

The potential due to a voltaic circuit through which a current of strength C is flowing is $-C\omega$, the negative sign being due to the fact that a positive current causes a negative potential. The positive direction of a current is that in which the hands of a watch move, but if the north pole of a magnet be brought near a circuit in which the current is flowing in this direction, it will be *attracted*, or negative work is done in moving a free north pole up to such a circuit. The potential is therefore negative, and, as in the same way experiments show that a current in the negative direction causes a positive potential, the potential due to a voltaic circuit must always have the opposite sign from that of the current. The equipotential surfaces and lines of force are similar in character and number to those of the equivalent magnet shell. The direction of the lines of force is important, and can easily be determined by the simple experiments referred to. A positive current in a circle attracts a north pole, and the positive direction is therefore towards that face on which the current appears to flow in the positive direction. Viewed from the other side of the circuit the current would appear to be negative, and on that side a north pole would be repelled. These relations are summed up by saying that the directions of the current and the lines of force are related in the right-handed cyclical order. A useful comparison is that commonly given by the use of a corkscrew. If the wrist be rotated to the right, the point of the screw advances. Letting the rotation of the wrist correspond to the movement of the current, the motion of the point will be that in which a north pole moves, or the positive direction of the lines of force.

As the relations proved for a magnetic shell hold in the case of a closed voltaic circuit, the work done in moving a pole m near a closed circuit in which a current of strength C was moving is $-C(N_1 - N_2)$, N_1 and N_2 representing as before the number of lines of force of the pole which are enclosed by the circuit. If the pole were fixed and the circuit moved the same work would have to be done, as the same forces of attraction and repulsion would have to be overcome through the same distance. This expression becomes, therefore, the formula for the work done in moving a closed voltaic circuit in a magnetic field. If N_1 is larger than N_2 the work done is negative,

or the circuit tends to place itself in a position in which it encloses the maximum number of lines of force. If the direction of the lines of force of the field is opposite to those of the circuit, the latter will, if free to move, first rotate so as to bring its own lines of force to correspond in direction to those of the field, and will then move in such a direction as to enclose the maximum number possible. It is seen in this connection that a maximum of negative lines is practically the same as a minimum of positive. The number of lines enclosed by the circuit depends on the inclination of its plane to the direction of the lines, varying in the ratio of the sine of the angle the lines make with the plane of the circuit. If this angle is called θ, the circuit tends to turn into a position in which $\theta=90°$, and the work it would do in turning, or the potential energy it possessed before movement, is measured by the expression $C(N_2-N_2 \sin. \theta)$. This principle is evidently of importance in its relation to electro-motors, but we must continue on another line of investigation.

If a closed circuit be placed in a magnetic field it will exhibit no tendency to move if no current be passed through it. If a current pass the wrong way, the circuit will, as we have seen, move to make the number of lines of force enclosed a maximum. In doing so it performs work, and this work must be derivable from the energy of the current. Part of this energy is dissipated as heat, and part expended in moving the coil. We have, therefore, t, being the time occupied in the movement,

$$CEt=C^2Rt+C(N^1-N_2) \quad . \quad . \quad (1)$$

or $$CR=E-\frac{(N_1-N_2)}{t}.$$

But CR is the E. M. F. remaining in the circuit, and is less than the original E. M. F. by the quantity $\frac{N_1-N_2}{t}$ which must also be of the nature of an E. M. F., but acting in opposition to that already existing. This expression is, therefore, the measure of E. M. F., induced in the circuit by its motion. (N_1-N_2) is the change in the number of lines enclosed by the circuit at the beginning and end of the time t. Hence we see that the E. M. F. induced in a closed circuit moving in a magnetic field, is measured by the *rate of change* of the number of lines of force inclosed, and is opposite in direction to that of the current which causes the motion. This result follows directly from the principle of conservation of energy, for if a current flowing in a given direction caused motion of the circuit, and this motion induced a current in the same direction as the original, it would increase the motion, and hence the energy of the system. Lenz' law, which says that if a current is induced in a magnetic field, it is in such a direction as to oppose by its electro-magnetic action, the cause inducing it is, therefore, directly deducible from the principle of conservation of energy.

We have thus far considered the case in which a current is flowing in the circuit at first, but the same rules apply in cases where there is no initial current. The formula $CR=E-\frac{(N_1-N_2)}{t}$, being true for all values of E, must hold when $E=O$, but in that case CR is the E. M. F. in circuit $=-\frac{(N_1-N_2)}{t}$ as before. This appears also from the fact that by diminishing t, by increasing the velocity of the circuit, the induced E. M. F. may be made to equal or overcome that previously existing, and there would eventually be a current flowing in the same direction, whether a current existed in the circuit at first or not. On the supposition that $E=O$ (1), becomes

$$O=C^2Rt+C(N_1-N_2) \quad . \quad . \quad (2)$$

or $$CR=-\frac{(N_1-N_2)}{t}, \text{ as above.}$$

As in this equation the original energy was zero, the energy of the induced current can be derived only from the work done in moving the coil, and if no external work is done by the coil, the energy thus applied appears as heat in the circuits.

The E. M. F. induced in the time t, being numerically equal to the change in the number of lines of force inclosed and acting by Lenz' law in such a direction as by its electro-magnetic action to oppose the cause inducing it, must tend to cause a current flowing in such a manner as to keep the number of lines inclosed by the circuit constant. If a coil be

moved so as to inclose a smaller number, the induced current must add enough to make the loss good; if moved so as to inclose a greater number, the induced current would cause enough to pass in the opposite direction to neutralize the gain. Consider the case of a coil moved along the lines of force, the observer looking along them in the positive direction. If, in the movement, the coil incloses a decreasing number, the induced current must make good the deficiency, and this by the rule already given for the relation between the direction of the lines of force and that of the current would be done by a right-handed or direct current, one flowing in the direction in which the hands of a watch move. If the coil moved inclosing an increasing number, the gain would be neutralized by the lines due to a left-handed or inverse current. Hence the rule given by Silvanus Thomson in his "Elementary Lessons"—"a decrease in the number of lines of force which pass through a circuit produces a current around the circuit in a positive direction, while an increase in the number of lines of force produces a current in the negative direction around the circuit." This rule is general, and is most useful, but in its application it must be remembered that the currents appear positive or negative to an observer looking along the lines of force in the direction in which a free north pole would move. Viewed from the other side they would of course appear to pass in the opposite direction. These relations, although somewhat difficult to reason out, are easily verified by experiment. It may be well to illustrate the application of the rules given. Suppose the lines of force to pass down perpendicularly through the paper, and a coil to be at rest in the plane of the paper. It encloses a maximum number of lines of force, and if rotated so that the right-hand edge comes to the front, while the left-hand goes behind the paper it will inclose a constantly decreasing number of lines, and a positive current will be induced. The E. M. F. will at first be small, as the rate of change is small, the edges of the coil moving almost along the lines of force. The rate will gradually increase until the coil has moved through one quadrant and is edge on to the observer, when, as the motion of the edges is at right-angles to the lines, the rate, and consequently the E. M. F. is a maximum. In this position the coil incloses no lines of force, and during the second quadrant it will move, inclosing an increasing number, and inducing, therefore, an inverse current. But the side of the coil now seen is the opposite to that in view during the first quadrant, and the inverse current is therefore in the same absolute direction in the coil as the former direct current. During the second quadrant the rate and E. M. F. decrease, becoming a minimum when the coil has completed a half revolution and is again in the plane of the paper. On entering the third quadrant, the number of lines inclosed decreases, and a direct current is induced; but as the same side of the coil is presented to the observer as in the second, the direction of the current is reversed in the coil. In the fourth quadrant the number of inclosed lines increases, but the other side of the coil is towards the observer, so that the absolute direction of the current is the same as in the third. The general direction of the current is therefore downward in that part of the coil in front of the the paper, and upward in the outer half; but as regards the coil itself, the direction of the current changes twice in every revolution, the point of change being where the circuit incloses the maximum number of lines of force. By the use of a commutator which shifts its connections at this point of the revolution, the current may be made to flow in one direction in the exterior circuit.

It now remains to trace the practical application of these results. In the formula for the induced E. M. F., let N denote $(N_1 - N_2)$, or be the change in the time t in the number of lines of force inclosed by the circuit. It is extremely difficult to obtain numerical values for N_1 and N_2, but it requires very little experimenting with magnets and iron filings to trace the general direction of the lines of force in any combination of magnets. All treatises on electricity or physics contain diagrams illustrating the distribution of the lines of force in different cases, and as the formula does not require any absolute value, but only a change or difference of values, this can be inferred with considerable exactness from an examina-

tion of the arrangement of the magnets in the field. From the formula, therefore, $E = \frac{N}{t}$ the following considerations are easily deduced:

1. The E. M. F. varies as the change in the strength of the field. If the coil passes from a position in which it incloses no lines, to another in which it incloses N lines, the latter should be as great as possible. If it moves alternately in a positive and in a negative field, the algebraic difference should be as great as possible. This relation is commonly given that the E. M. F. varies as the strength of field, but it is possible to move a coil in the most intense uniform field without inducing any current.

2. The E. M. F. varies as the velocity. If N lines are cut in a time t, the shorter t the greater the E. M. F. Theoretically, therefore, the E. M. F. of a dynamo varies as the speed, and this relation is so nearly true in practice as to be always assumed in calculation.

3. The formula gives the E. M. F. induced in a single circuit. In a coil of n turns each incloses the same number of lines of force, and each would therefore induce the same E. M. F. But the turns being in series the total E. M. F. in the coil would be n times that in one turn. If each of the n turns were moved in the field separately, n times the work required to move one turn would be done, and this amount of work must also be done when the whole coil is moved at once. The E. M. F. varies, therefore, with the number of turns in the moving coil, but as the outer turns inclose more lines than the inner, the ratio of gain would be slightly greater than the number of turns.

4. In a coil rotating with uniform angular velocity, the rate of change of the lines of force inclosed would be greatest where the coil cuts them at right-angles, or where the plane of the coil is parallel to the lines of force.

5. The larger the coil the greater the number of lines passing through it, and other conditions remaining the same, the E. M. F. varies as the area of the coil. A difficulty exists in making the armature larger and keeping the intensity of the field constant. Pole pieces are frequently used to accomplish this end, but may work disadvantageously.

Perfect working in a dynamo requires a constant change of E. M. F., and consequently a constant rate. If a large coil should revolve between the poles of two bar magnets, and no iron were present to modify the distribution of the lines of force in the field, the greater part would pass directly between the poles, and would be cut during a small part of the revolution of the coil, during which time the rate and induced E. M. F. would be high, but in other parts of the revolution the rate would be very small. The available E. M. F. would be induced suddenly, but the sudden creation of a current causes high self-induction and temporary strong extra currents, which in a dynamo are not only prejudicial but dangerous, on account of the high E. M. F. they may have. Idle wire in the armature also reduces the current. It is therefore desirable to prevent a concentration of the lines of force in a small part of the field. In a coil rotating in a uniform field, the advantage of the constant rate is attained by the change of the number of lines inclosed in the ratio of the sine of the angle between the plane of the coil and the direction of the lines, and as a field tends to become uniform would this advantage be gained. Pole pieces, by increasing the magnetic surface, operate favorably, but if they encircle the armature so closely as to approach each other nearly, lines of force will pass from one to the other which are not cut at all by the coil in its revolution. Experiments have shown that in some cases the E. M. F. is increased by cutting off the edges of the pole pieces. Edison has apparently carried this advantage to the utmost, the long magnets and immense pole pieces of his machines tending to secure a field more nearly uniform than in other types.

In an improved bichromate of potash battery M. Luigi Ponci uses a liquid thus made: One kilogramme of bichromate is crushed and dissolved in 4 litres of boiling water, and to this 2 litres of chlorhydric acid is added. A liquid is thus obtained containing chloride of potassium and bichromate of potash, which prevents the formation of crystals in the battery.

THE MATTER OF SPACE.

From "Nature."

Of late years there has been a growing tendency towards the belief that matter is present everywhere throughout the universe, as well in interstitial space as in the bodies of the spheres. Yet an older hypothesis is still widely held. The phenomena of light seem to require some substantial medium in space, but this substance has been viewed as specifically distinct from matter, and named ether. Another class of thinkers has devised still another species of substance. This is required to meet the demands of the new gravitation hypothesis, and consists of excessively minute particles, moving with intense speed, and pressing vigorously on the larger and slower particles of matter. In the past, still other species of substance were imagined; heat, electricity, etc., were each ascribed to a specifically distinct substance.

Now, however, the tide has turned, and the inclination is to believe in only a single form of substance. There are, of course, countless distinct conditions produced by the aggregations of substance, and variations from simplicity to complexity, but this may not necessarily require more than a single kind of basic particle, or whatever we may call it. If the substantial contents of space are similar in constitution to the matter of the spheres, their state of existence must be much more simplified. In the spheres we have matter ranging from the simple elementary gasses of the atmosphere, through the complex mineral compounds of the solid surface, to the highly compounded organic molecules. In outer space the variation is probably in the opposite direction, and substance may exist there in a condition much more highly disintegrated than the atmospheric gases. This view is not held by all theorists. Dr. Siemens argues that space holds molecules of considerable intricacy, comprising certain terrestrial elements, and their simpler compounds; as to the contents of space we know that there are very numerous solid masses, some of considerable size, others minute, and possibly ranging through many degrees from the largest to the minutest. Yet these really occupy but an inconsiderable portion of space, and apparently originated in solar or planetary orbs.

Such is, briefly stated, the state of knowledge and of hypothesis concerning the substantial contents of space. We need but add the uncertain reasons for arguing the presence of a resisting medium in space, and the necessity of a highly elastic condition of the light-conducting substance, to exhaust the subject so far as yet pursued.

It is held by some that the gravitation energy of the suns and planets is sufficiently great to sweep space of all contiguous material particles, except those solid masses which are saved from this fate by the vigor of their orbital motions. The atmospheres of suns and planets are retained with an energy very greatly in excess of their reverse energy of molecular motion, and therefore it is quite impossible that any of this material should escape into space, or that any similarly-conditioned material should exist contiguous to the spheres, without being forced to become atmospheric matter. The centrifugal energy of the earth's atmosphere at the equator is only $\frac{1}{171}$ of that necessary to overcome gravity. The molecules of the atmosphere have also a vigor of heat vibration about equal to their centrifugal energy. Hence the resisting energy of these molecules is far below the gravitative energy, and they are vigorously held.

The question of the possible existence of gravitating matter in interspheral space depends strictly upon that of its motor energy. If the momentum of any particle, or of the whole sum of particles, be insufficient to constitute a centrifugal energy equal or superior to the centripetal energy of gravitation, then the material contents of space must inevitably be drawn into the attracting spheres, as atmoshperic substance, and space be denuded of matter. If, on the contrary, the centrifugal energy of these particles be sufficient to resist gravitation, they will remain free, and space continue peopled by matter.

Such gravitative particles, wherever

existing in space, could not be for an instant free from the influence of spheral attraction whatever their energy of motion. If this energy be too small, they must be related to the spheres as falling bodies, and must become atmospheric matter. If the two opposite energies be equal, they must be related to the spheres as planetary bodies, and circle in fixed orbits around the center of attraction. If the centrifugal energy be in excess they must assume the condition of independent cometary bodies, temporarily influenced but not permanently controlled by any sun, and wandering eternally through space.

Such are the three possible conditions of the material contents of space. If the first obtain, space must be denuded of matter; if the second obtain, it will permanently contain matter in a partially elastic state; if the third obtain, it will permanently contain matter in a highly elastic state, since the pressure upon each other of the vigorously centrifugal particles must be great and may be extreme. Of course no single particle could long retain its direction of motion, as related to any sphere. Constant impacts must constantly vary the directions of molecular motion. But the motion of each particle is successively transferred to a long series of particles, and thus is virtually continued in force and direction. Each motion pursues its course independently, though not as affecting any fixed particle of matter, and each particle aids in the progression of a vast network of motions, proceeding in every direction throughout the universe. Thus each particle, though not actually changing its place, may have motor relations which extend in every direction to the utmost extremes of space. It is a node in an interminable network of motions, and its incessant leaps throughout the limits of its narrow space are each part of a long motor line, which affects successively myriads of particles. So far as the energy of gravitation is concerned the effect upon this incessantly transferred motion is precisely the same as if the motion was confined to a single particle. If it lack energy the motion will be a falling one; if it equal the gravitative energy it will form a closed orbit. If it exceed the gravitative energy it will form an open curve, and be only temporarily controlled by any sphere.

In this interchange of motor energy certain particles may continually decrease in vigor of motion, and if near solar orbs may be drawn in as atmospheric matter. But they can only lose motion by transferring it to others, which would in consequence become more independent of gravity. The sum of motor energies in the universe must persist unchanged, and the aggregation of atmospheric substance around any planet must cause an outflow of motor energy which will increase the motor vigor of exterior particles. In such a case the height of atmosphere in any sphere will depend partly on the attractive vigor of the sphere, and partly on the average motor vigor of the whole sum of matter. Every contraction and loss of motor energy by any portion of matter will increase the motor energy of remaining matter, and a fixed limit to the atmospheric control of every sphere must result, since in the outer layers of its atmosphere the centrifugal energy of molecular motion must increase until it equals the energy of gravitation.

Can we arrive at any conclusion as to which of the three possible conditions above considered really exists? If so we can answer the question as to the existence of matter as a constant tenant of space, and also reach some conclusions as to the character of its motor conditions.

There is one line of thought which seems to lead to a settlement of this question. If the nebular hypothesis of the formation of solar systems be accepted as true, either wholly or partly, there can be no doubt as to the interspheral status of matter. The conditions of nebular aggregation indisputably settle it.

This hypothesis holds that the matter now concentrated into suns and planets was once more widely disseminated, so that the substance of each sphere occupied a very considerable extent of space. It even declares that the matter of the solar system was a nebulous cloud, extending far beyond the present limits of that system. From this original condition the existing condition of the spheres has arisen, through a continued concentration of matter. But this concentration

was constantly opposed by the heat energy of the particles, or, in other words, by their centrifugal momentum. This momentum could be only got rid of by a redistribution of motor energy. If, for illustration, the average momentum of the particles of the nebula was just equivalent to their gravitative energy, then a portion of this energy must radiate or be conducted outwards ere the internal particles could be held prisoners by gravitation. The loss of momentum inwardly must be corelated with an increase of momentum outwardly.

This is a necessary consequence of the heat relations of matter. As substance condenses its capacity for heat decreases, and its temperature rises, hence a difference of temperature must constantly have arisen between the denser and the rarer portions of the nebulous mass, and equality of temperature could be restored only by heat radiation. This radiation still continues, and must continue until condensation ceases, and the temperatures of the spheres and space become equalized, but this is equivalent to declaring that as the particles of the spheres decrease in heat momentum those of interspheral space increase, and if originally the centrifugal and centripetal energies of matter approached equality, they must become unequal, centripetal energy becoming in excess in spheral matter, centrifugal energy in the matter of space. Thus, as a portion of the originally widely distributed nebulous matter lost its heat, and became permanently fixed in place by gravitative attraction, another portion gained heat, became still more independent of gravity, and assumed a state of greater nebulous diffusion than originally. The condensing spheres only denuded space of a portion of the matter which it formerly held, and left the remainder more thinly distributed than before. The spheres, in their concentration, have emitted, and are emitting, a vast energy of motion. This motor energy yet exists in space as a motion of the particles of matter, which therefore press upon each other, or seek to extend their limits with increasing vigor, so that the elasticity of interspheral matter is constantly increasing.

It might be hastily imagined that such an excess of heat vigor in the matter of space over that of the spheres should declare itself in temperature. But it must be remembered that temperature is no measure of the absolute heat contents of matter.

Condensation increases, rarefaction decreases, temperature with no necessary change in absolute heat contents. The expression "fire mist," so often applied to the matter of uncondensed nebulæ, gives a very erroneous impression. The matter of the solar system nebula, though containing a high degree of absolute heat, was probably of low temperature. Its great rarity must certainly have greatly decreased its temperature. As a differentiation in this matter took place, one portion becoming condensed, another portion more rarefied, the former must have increased, the latter decreased, in temperature. Eventually the extreme condensation of one portion of this matter, and rarefaction of another, caused an extreme difference in temperature. An excessive radiation from the spheres to space has taken place in consequence, the absolute heat of the former constantly decreasing and that of the latter increasing. But the difference in temperature still continues great, the influence producing it acting much more rapidly than the influence tending to obliterate it. Eventually an equality of temperatures may be produced, but only by the production of a very considerable inequality of absolute heat. This must be the final result of spheral condensation and nebulous rarefaction of exterior matter; namely, equalization of temperature, with a change from the original homogeniety to a great heterogeniety of heat contents.

But we are again brought back to the question of the motor energies of matter. Are they sufficiently great to enable a portion of this matter, when reinforced in motor energy by radiations from the spheres, to defy gravitative attraction and remain free in space? Undoubtedly so, and much greater than would be simply requisite for the purpose, since we find the matter of the planets, after their immense losses by radiation, still possessed of a considerable excess of motor energy. The earth, for instance, has an orbital motion sufficient to maintain it at a considerable distance from the sun. But the motion of the earth is but the combined

motion of its molecules. This motion once existed as independent molecular motion, which in time, under the influence of gravity, became dependent molecular motion. We have already spoken of the fact that the particles of space, in consequence of their heat motions, tend to dart off in straight lines of motion, except in so far as the gravitative attraction of spheres causes these lines to become curved. These lines of motion, so far as individual particles are concerned, are checked by the particles coming into contact with others. The motion, however, proceeds onwards, though it is carried by successive, instead of by single particles. If, however, a number of particles move in company in the same direction, they may move much further as individuals, before transferring their energies. And if an immense mass of particles come to thus move in company their individual excursions may be indefinitely extended. The lines of motion, instead of being continued by successive particles, are continued by the same particles, and molecular motion becomes mass motion. The motion of terrestial molecules, in their revolution around the sun, resemble those of the molecules in Prof. Brooks vacuum tubes, constituting his "fourth state of matter."

Now the degree of resistance of such a mass to centripetal energy will indicate the degree of resistance of the original uncombined molecules. In the earth the motion of the molecules, thus combined, yields a centrifugal energy sufficient to maintain the earth at its present distance from the sun. But this is only a portion of its molecular energies. Its molecules possess considerable independent motion, and form nodes in lines of radiation that extend in every direction. They have also lost a great vigor of motion by radiation to space. It follows that the original momentum of these molecules must have constituted a centrifugal vigor greatly in excess of their centripetal vigor. It secondarily follows that the momentum of those molecules of the nebula which still exist in space, augmented as it has been by radiations from the spheres, yields a very energetic excess of centrifugal vigor. Many of the comets have a centrifugal energy in excess of the centripetal energy of the sun, yet this represents only a fraction of the energy of their molecules, and a much smaller fraction of the energy of the material particles of space.

The combination of the centrifugal energies of terrestrial particles is due to the fact of a secondary center of gravity having been formed. The heat velocity of its particles, in excess of that displayed in their revolution around the sun, has become partly a revolution around the earth's axis, and is partly retained as heat vibration. But the heat velocity of the material particles of space is not thus secondarily employed. It is affected by the attraction of the sun, or of the nearest sphere; but evidently, from the considerations above taken, this attraction cannot be sufficient to over-balance the centrifugal energy and cause atmospheric aggregation or even to cause orbital revolution. The particles must have energy sufficient to make them independent of spheral gravity. Their straight lines of motion must become to some degree curved in response to gravity, but cannot become closed curves. Instead of becoming planetary, they remain cometary lines, of very open orbit. For if we imagine the earth to be suddenly restored to its nebulous condition, or its particles to be set free in space, they would possess a velocity of motion much in excess of the earth's orbital velocity. Hence they could not be controlled by the sun. The existing particles of space possess a still much greater velocity, and are therefore much more free from gravitative control.

Certain necessary results of this condition have been considered. The lines of centrifugal motion in space are not confined to single particles as in the earth, but are transferred from particle to particle. The effect, however, is precisely the same; this motion of successive particles is in no respect different in effect from what we would have if a single particle were free to move in the same direction. Each particle moves a certain distance, and then transfers its motion in that direction to another. But it immediately pursues some other direction of motion in response to impact, and this aids in the progressive movement of innumerable lines of motor energy. The great centrifugal vigor of these motions must cause an energetic compressing influence upon interspheral matter, and thus produce an

elasticity, sufficient perhaps for the requirements of light radiation.

The lines of motion thus transferred through space cannot be unvarying in their orbital directions. Nature knows no great or small in her processes, and each moving particle of the free matter of space is controlled by the same principles which control the motions of a planet. It is subject to perturbations from lateral attractions, similar to those which draw planets and comets out of their orbits, and completely change the orbit of the latter. And its impacts with other particles yield effects such as would arise in impacts between planets of oppositely moving systems. Action and reaction are equal, in this as in every case. The orbit and the speed of a line of motion may be changed through impact or attractive resistance, but only by its causing an opposite change in some other line. Thus the lines of motor energy referred to are not unvarying in speed and direction, but are unvarying in their sum of correlated speeds and directions. The variations which take place in the orbits of spheres and comets through attractive perturbation, and the greater variations which would take place did spheres come frequently into contact, are precisely similar to those which must occur in the case of interspheral particles, and any change in the direction of one orbit is balanced by an equal opposite change in the direction of another orbit, the balance of motor direction and energy in nature being exactly preserved.

If such a line of motion pursues a cometary ellipse and enters the atmosphere of a globe, it must be affected by friction precisely as if the line of moving particles were a single particle, or a minute comet. It might be obliterated by friction or resistance, as the orbital motion of a falling body is obliterated. But this obliteration is really caused by the opposing energy of opposite lines of molecular motion. The single line of motion may be distributed into a thousand lines differing in direction, but the component of these thousand lines must agree with the original line.

The transfer of motion from particle to particle here indicated may take place through attractive resistance as well as through impact resistance. The original disintegration of the matter of space must have increased, as spheral condensation denuded space of much of its material, and as radiation from the spheres increased its motor energy. If matter thus divided up into smaller and smaller particles, these may have continued as closely contiguous in space as are the molecules of spheral atmospheres. In such a case they may present the conditions of excessive rarity so far as weight of matter is concerned; of close contiguity of particles, sufficient to permit the exercise of attractive energy; of great compression, through their vigor of centrifugal motion, and of intense elastic resistance to compression. These are the conditions necessary for the transfer of the radiations of light and heat. In these radiatures motion is conveyed through space by transfer of vibratory motions, not of impacts. The vibrating particle swings between lateral chains of attraction, and causes a like transverse swing in successive particles with which it is attractively connected. Greater energy here causes only greater width of vibration, not greater rapidity of transfer. The latter depends only on the elasticity of the matter concerned. Impact transfer of motion, on the contrary, must differ in speed with every difference in vigor. It is transferred by the motions of what we know as local heat, similar to the incessantly varied heat motions of gaseous matter. As the particles are unvarying in weight, increased momentum can be gained only by increased rapidity of motion, and the lines of motion thus transferred through space vary in speed with every variation in vigor. Every motion, of every particle of matter, is really a minute portion of an orbit, which represents that of a falling body, of a planet, or of a comet, according to its rapidity. Though the momentum affects successive particles of matter the orbit is continuous, except to the extent that it is varied by perturbations through attraction and impact.

Wherever any influence aids a translation of interspheral matter—causes a wind to blow through space—the lines of motion continue to be conveyed by the same particles. The orbital motions of the spheres are such winds through space; minor aggregations of moving matter may enter the atmosphere of the

sun or other globes. But no atmosphere can become permanently increased in this manner; such masses, checked by friction, must yield motion, which flows outward. The centrifugal energy of the molecules of the external atmosphere is thereby increased, and the gain of matter must be balanced by an equal escape of matter at that critical atmospheric limit where centrifugal and centripetal energies are in balance. But any such fall of interspheral matter must aid the radiant emissions of the sun. Its loss of proper motion, its high degree of absolute heat, its increased temperature through condensation, and its consequent radiation, would make it a source of solar heat. Any such cometary matter must form part of "The Fuel of the Sun."

ON THE MOLECULAR RIGIDITY OF TEMPERED STEEL.

By PROF. D. E. HUGHES.

A Paper read before the Association of Mechanical Engineers.

During the course of some recent researches the writer has been enabled, by the aid of the induction balance, to perceive some remarkable molecular differences between the constitution of iron and of steel. There are numerous papers in the *Comptes Rendus*, from 1830 to 1850, in which it is suggested that tempered steel is a true alloy of iron and carbon, the carbon being present in varying degrees according to the temperature at which the alloy was formed, and being afterwards rendered permanent by sudden cooling. In a late discussion on this subject the writer made a few remarks, in which he pointed out the marked difference between softened and tempered steel, as to solubility in dilute sulphuric acid, and expressed the opinion, formed from these and many previous experiments, that tempered steel was a true alloy. He has since continued these experiments, not, however, to prove the chemical composition of tempered steel, but to investigate its peculiar molecular structure, as indicated by the induction balance. The apparatus necessary to perceive the effects of stress or torsion, as described in this paper, is exceedingly simple. Suppose, for instance, that we take an ordinary single-coil electro-magnet, and join its terminals with that of a telephone or sensitive galvanometer. If we now pass a current from a battery through the iron core alone of the electro-magnet, we have a sharp click at each make and break of the current. This effect was discovered by Page, and fully described by De La Rive. If we keep the current passing constantly through the core, we have no effect; but if we then give a slight torsion or twist to the core, either to the right or left, we at once hear a sharp click; and if we keep the torsion constant, and then make frequent interruptions of the battery, we have a greatly increased sound at each make or break, indicating a greatly increased force of electric current. In order to investigate this phenomenon, the author constructed a special though very simple apparatus. A coil, having a large aperture, is fixed to a board; two small abutments or supports, at a few inches distant on each side of the coil, allow us to suspend or fix an iron wire passing through the aperture, which then becomes the core of an electro-magnet. This forms the essential portion of the apparatus. The iron or copper wire rests upon the two supports, which are 20 centimeters apart; at one of these it is firmly clamped by two binding screws, while the opposite end can turn freely. The wire is 22 centimeters long, projecting 2 centimeters beyond its support. On the projecting end is a key or arm, which serves as a pointer moving on a graduated circle, and gives the degree of torsion which the wire may receive. A binding screw allows us to fasten the wire, after turning the pointer to any degree of torsion, and thus preserves the required stress as long as is necessary. The exterior diameter of the coil is $5\frac{1}{2}$ centimeters, and that of the interior vacant aperture is $3\frac{1}{2}$ centimeters: the width is 2 centimeters. Upon this coil is wound 200 meters of No. 32 silk-covered copper wire. This coil is fastened to a small board, so ar-

ranged that it can be turned through any desired angle in relation to the iron wire which passes through its center; and it can also be moved so as to lie over any portion of the 20 centimeters length of wire, in order that different portions of the same wire may be tested under a similar stress. The whole of this instrument, as far as possible, should be constructed of wood, in order to avoid all disturbing inductive influences of the coil upon other pieces of metal. The iron wire at its rear or fixed end is joined to or makes contact with a copper wire, which returns to the front part of the dial under the board and parallel to the coil, thus forming a loop. The free end of the iron wire is joined to one pole of the battery; the copper wire under the board is joined to a rheotome, and thence to the other pole of the battery. The coil is joined to a telephone or a sensitive galvanometer; and we may either pass the current in the manner described, or may reverse all the communications, passing the current through the coil instead of the wire, and listening with the telephone to the induced currents upon the iron wire alone.

In order fully to understand the phenomena which takes place, we must bear in mind Faraday's discovery of electric magnetic induction—namely, that any wire conveying an electric current induces in general a momentary secondary current in any independent circuit whose wires are parallel to it; the effect being at its maximum when two wires are parallel, diminishing as the angle of these wires is increased, and at 90° being absolutely zero. Consequently, when we place a copper wire in the axis of the coil, with the above apparatus, and pass a current through this wire, we find no effect whatever, no trace of induced currents; simply for the reason that this copper wire crosses all of the wires of the coil at an angle of 90°. We also find that no effect takes place upon torsion being applied to the copper wire. If we now place a small rod of iron parallel with the conducting copper wire, we have no effect; but if the iron rod is turned at an angle to the wires a current is observed, the force increasing from parallelism to an angle of 45°, and decreasing again from this angle to 90°, where we have again no effect. The conducting copper wire thus induces electric magnetism in the iron rod, and this magnetism reacts upon the coil; but this only holds as long as the rod is not parallel to either coil. At an angle of 90°, although at its maximum of electric magnetism, the iron rod becomes parallel to the coil upon which it reacts; consequently we have again a zero of current. In place of one rod, we may insert several short rods; and if these are all turned together in the same direction we have similar effects. Knowing this, we can understand that if each molecule of a rod were endowed with separate magnetic power, and if we could cause these to rotate through any angle round the axis, we might expect similar reactions to those of the small separate iron rods already mentioned. If we replace the copper wire spoken of by an iron wire, and send intermittent currents through it, we still have no induced current upon the coils; but the instant we apply a very slight torsion, say 10 or 20 per cent. of one turn, we at once perceive strong induced currents. These are positive for right hand torsion and negative for left-hand torsion. Thus we can not only produce induced currents, but, without changing the direction of the primary electric current, we can change the induced currents, making them positive or negative as we please—exactly as would occur if we rotated in opposite directions the small iron bars, placed side by side with the copper wire. At this point it becomes important to know if these effects are produced by the twist given by torsion to the whole mass of the wire, or if each molecule turns separately and independently around its axis. There are many proofs that the latter view is correct; for, assuming the former, then, if an iron wire be twisted permanently by 30 or more entire turns, we should expect greatly increased effects as compared with those given by 10 or 20 per cent. of a single turn. But we find that, after the first instant of torsion, we have no increase of force in the current, even with a molar twist of 30 whole turns, which must, of course, produce a certain molecular twist; we find that the slightest torsion, say of 10 per cent, backwards, is sufficient to reverse the current, and thus more than neutralize the whole inclination which had been given to the molecules by the permanent torsion. Again, if, whilst the iron wire is under the influence of

torsion, we bring near it one pole of a large natural magnet, laid in the direction of the wire, we find that the currents gradually diminish, until, when the magnet touches the wire, we at last produce zero. The polarized molecules, which under the influence of torsion lay at a certain angle with the axis, have thus been caused to rotate back again and become parallel. Again if we approach the same pole with the magnet at right angles to the wire, we find that the current gradually returns to zero (and, therefore, the molecules to parallelism) when the magnet is about two inches; but on bringing the magnet still nearer, they pass the zero point, now giving increased reversed currents, until they reach a maximum, when the magnet is close to the wire. We have thus rotated the molecules from their original angle of torsion, say of 45° to the right through zero to 45° to the left. If this view is correct, we should expect that we might produce electric currents of reversed directions without the aid of any battery, by simply giving a to and fro torsion to the wire; and this proves to be the case. For we may join the telephone either to the exterior coil or to the simple circuit of the wire, and we shall then hear a sharp click at each movement of torsion to the right and left, thus imitating and reproducing all the effects which would be obtained by rotating a separate magnet through different angles of inclination with the wire. There are many proofs which confirm this view*; but as the object of the author is to show the remarkable difference which exists between iron and steel in this respect, he will confine himself to showing the very great apparent rigidity of the molecules of tempered steel as compared with those of iron.

A very remarkable difference appears when we turn to tempered steel. For here we find that at certain degrees of temper (*e.g.*, that known as blue or spring temper) there are only slight traces of molecular disturbance or rotation, no matter how many mechanical turns or twists we may put on the wire. In fact the molecules here seem fixed and homogeneous throughout the mass. We have perfect molar elasticity, but no traces of rotation of one part over another—in other words, no molecular elasticity. Thus in iron we have an elasticity due solely to the freedom of molecular motion. In hard steel, on the contrary, we have but slight molecular freedom, with great molar elasticity, in which the separate molecules do not rotate separately but all as one mass. It is necessary to point out this difference of molar rigidity as shown in tempered steel and in iron, because tempered steel is not the only form which thus differs in its mechanical and physical qualities from iron or soft steel. A similar difference is shown also by several known alloys of iron. We can decrease the apparent rigidity of steel by the application of heat; for if we pass a constant and powerful current through the steel wire, which previously gave but feeble traces of rotation, and then heat this wire to a red heat, a strong induced current is gradually produced. The current here has the power of rotating the polarized and heated molecules, and so giving out comparatively strong induced currents. But on cooling this wire it is impossible again to reduce it to silence. The molecules remain rigid, but at an angle to the axis. With iron, however, upon the application of heat under the same circumstances, we have a most violent rotation, which entirely disappears on cooling—proving again the great comparative freedom of its molecules. We might believe that all the above effects in steel are due, not to the rotation of the molecules, but to the more or less retentive or "coercitive" force of steel with regard to permanent magnetism. But coercitive force, while it may suffice to explain, according to accepted views, the retention of magnetism, does not explain why we can produce positive and negative currents by right or left-handed torsions, nor why we should have induced currents by torsion. If we accept the term coercitive force as simply applicable to each molecule, then we have still to consider the greater freedom of motion of these molecules in iron than in steel. It is a general belief (which the author has hitherto shared) that the molecules of tempered steel have far greater coercitive force than those of iron. A simple experiment will, however, prove this not to be the case. For if we suppose that the molecules of iron turn with far greater freedom, it follows that they should also

* "Molecular Magnetism," by Professor D. E. Hughes. Proceedings of the Royal Society, March 7–17 and May 10, 1881.

turn by the application of far less force. Now if we take a soft iron and tempered steel wire, and place them at a given distance from a suspended magnetic needle, after finding them both to be free from magnetism, and we then magnetize these wires by drawing them over the poles of a natural magnet, then we may no doubt find as usual that the tempered steel has a far greater amount of remaining magnetism. But if, instead of this, we limit the reactive force of the natural magnet by placing a piece of wood, say ½ inch thick, between the magnet and the wire magnetized, thus limiting and controlling the force to any degree, according to the interval between the magnet and the wire to be magnetized, we then find on magnetizing these two wires with a weak reactive force, and again observing its action upon the needle, that the soft iron still shows powerful retentive or coercitive force, whilst the tempered steel has but feeble traces of magnetism, or none at all. Thus, contrary to the author's previous convictions, it appears that iron possesses more coercitive force than steel whenever the inducing force is limited, and within the range of iron. If iron merely possessed greater coercitive force than steel, it would be impossible for us to employ soft iron in electro-magnets requiring quick changes of magnetism. But although, in the previous experiment, the remaining magnetism was far greater in the iron than steel, yet the magnetic force of the iron, whilst under the influence of permanent magnet, was some twenty times greater than its remaining magnetism; whilst with the steel there was but a slight difference in the force developed whilst it was under the feeble influence of the natural magnet, and when this was withdrawn.

Assuming the freedom of motion of the molecules to be greater in iron than steel, it occurred to the author that he should be able to free the soft iron from its remaining magnetism by simple vibration of the wire. This was found to be the case. An iron and steel wire are magnetized to saturation, or both may be given the same amount of permanent magnetism. We will suppose that they both deflect the suspended needle through 40°. Now taking the steel wire and fastening one end in a brass vice, give its free end a slight pull to set it in vibration: it will be found that the steel has lost but 2°, having still 38° of permanent magnetism, which cannot be further reduced by repeated vibrations. The instant, however, that a similar vibration is given to the soft iron wire, its remaining magnetism nearly all disappears; there is left at most 2°, or in some cases only a trace. Thus the molecules are seen to be so comparatively free in iron that mere vibration will aid them in rotating. These two wires were again observed vibrating whilst under the influence of the permanent magnet. There was then a greater magnetic effect produced in the iron wire than previously, the vibrations aiding the rotations produced by the natural magnet. The author was desirous to render this freedom of iron and rigidity of steel, so that these effects might be actually seen. For this purpose we may take three glass tubes, or ordinary phials, of any length or diameter, say 10 centimeters in length by 2 centimeters in diameter. If we now put iron filings in these tubes, leaving about one third vcaant, so as to allow complete freedom in the filings when shaken, we find that each tube, when magnetized, retain an equal amount of residual magnetism, and that this all disappears upon slightly shaking the tube; we are thus imitating the effects of vibration. But if in one of these tubes we pour melted resin (or in fact any slightly viscous liquid, such as petroleum, suffices) we then render these filings more rigid, and then we can no longer produce by shaking the disappearance of its residual magnetism. In pouring in petroleum we have apparently been introducing a strong coercitive force; but we know that it can only have the mechanical effect of rendering the iron filings less free to turn, and so comparatively rigid. If we desire to see the effect of torsion, we have only to shake the filings so that when the tube is held horizontal the vacant space is above, and rotate slightly (but without shaking) the tube containing the free filings about a horizontal axis. Its remaining magnetism instantly disappears upon rotation, although we evidently have not changed the longitudinal position of its particles. A similar effect takes place upon a soft iron wire, for if we magnetize it and observe its remaining magnetism, we find that upon giving a slight torsion to this wire, its remaining

magnetism instantly disappears—a similar effect to that in the rotating tube of iron filings.

The author has remarked in these researches that in all alloys of iron the molecules are far more rigid than in the pure metal and further that, with steel, tempering adds greatly to this rigidity. He is now engaged upon the question of the effect of different tempers on the same steel, and hopes in a future paper to be able to bring the results before the Institution. Soft steel, when compared with hard drawn iron, shows that the mechanical hardening of iron has not in any great degree diminished its molecular freedom. Even the softest steel shows a high degree of molecular rigidity, as compared with the hardest iron, but far less than that of tempered steel. This would seem to indicate that steel in its softest state is still an alloy, though only feeble quantities of carbon may be held in that condition. We thus perceive that a great physical change takes place in iron upon the slightest alloy with carbon; and that tempering produces this change in its highest degree. The writer therefore is strongly in favour of the view propounded long since, that steel when tempered is an alloy, containing fixed carbon in a far greater quantity than when soft. We know the physical properties of magnetic oxide of iron, of iron and tungsten, and of iron and sulphur. Now in all these the writer has found that the iron loses its molecular freedom when even slightly alloyed. The physical results are therefore the same as those produced in tempering steel; and the induction balance thus indicates strongly that tempered steel shows the characteristics of a true alloy. We could not have such a great physical difference between iron and steel, as above noticed, except by corresponding changes in its mechanical properties; and it is with a view of bringing out these relations in a discussion on this point, that the author has ventured to bring his views before the Inst. of Mechanical Engineers.

ELECTRICITY APPLIED TO EXPLOSIVE PURPOSES.

By PROF. F. A. ABEL, C. B., F. R. S., Hon. M. Inst. C. E.

From "The Engineer."

In introducing the subject the lecturer indicated the principal advantages which it had been early observed would result from a certain mode of firing explosive charges by electric currents instead of by the ordinary fuses, the best of which had inherent defects, greatly limiting their use for any but the simplest operations. He traced the history and development of electric firing from the crude experiments of Benjamin Franklin, about the year 1751, through the various stages in which frictional electricity, volta-induction apparatus, and magneto-electric machines had supplied the means of generating the current, the tendency of late years being to revert to a modified form of voltaic battery for one class of work, and to employ dynamo-electric machines for another class. The history and development of the low tension, or wire fuse, and of the various fuses employed with electric currents of high tension were also discussed, and their relative advantages, defects, and performances were described. The only sources of electricity which at present thoroughly fulfilled the conditions essential in the exploding agent for submarine mines were constant voltaic batteries. They were simple of construction, comparatively inexpensive, required but little skill or labor in their production or repair, and very little attention to keep them in constant good working order for long periods, and their action might be made quite independent of any operation to be performed at the last moment.

When first arrangements were devised for the application of electricity in the naval service to the firing of guns and so called outrigger charges, the voltaic pile recommended itself for its simplicity, the readiness with which it could be put together and kept in order by sailors, and the considerable power presented and maintained by it for a number of hours. Different forms of pile were devised at Woolwich for boat and ship use, the latter being of sufficient power to fire heavy

broadsides by branch circuits, and to continue in serviceabfe condition for twenty-four hours, when they could be replaced by fresh batteries, which had in the meantime been cleaned and built up by sailors. The Daniell and sand batteries first used in conjunction with the high tension fuse for submarine mining service were speedily replaced by a modification of the battery known as Walker's, which was after some time converted into a modified form of the Leclanche battery. The importance of being able to ascertain by tests that the circuits leading to a mine, as well as the fuses introduced into that circuit, were in proper order, very soon became manifest; and many instances were on record in the earlier days of submarine mining of the disappointing results attending the accidental disturbance of electric firing arrangements, when proper means had not been known or provided for as certaining whether the circuit was complete, or for localizing any defect when discovered.

The testing of the Abel fuse, in which the bridge or igniting and conducting composition was a mixture of the copper phosphide and sulphide of potassium chlorate, was easy of accomplishment—by means of feeble currents of high tension—in proportion as the sulphide of copper predominated over the phosphide. Even the most sensitive might be thus tested with safety; but when the necessity for repeated testing, or even for the passing of a signal through the fuse, arose, as in a permanent system of submarine mines, the case was different, this fuse being susceptible of considerable alterations in conductivity on being frequently submitted to even very feeble test currents, and its accidental ignition, by such comparatively powerful test or signal currents as might have to be employed, became so far possible as to create an uncertainty which was most undesirable.

Hence, and also because the priming in these fuses was liable to some chemical change detrimental to its sensitiveness, unless thoroughly protected from access of moisture, another form of high-tension fuse, specially adapted for submarine mining service, was devised at Woolwich. This, though much less sensitive than the original Abel fuse, was sufficiently so for service requirements, while it presented great superiority over the latter in stability and uniformity of electric resistance; and, though not altogether unaffected by the long-continued transmission of test currents through them, the efficiency of the fuse was not affected thereby. Although high-tension fuses presented decided advantages in point of convenience and efficiency over the earlier form of platinum wire fuse, the requirements which arose, in elaborating thoroughly efficient permanent systems of defence by submarine mines, and the demand for a battery for use in ships which would remain practically constant for long periods, caused a very careful consideration of the relative advantages of the high and low tension systems of firing to result in favor of the employment of wire fuses for these services. In addition to the disadvantages pointed out there was an element of uncertainty, or possible danger, in the employment of high-tension fuses, which, though fully eliminated by the adoption of voltaic batteries, in place of generators of high-tension electricity, might still occasionally constitute a source of danger, namely, the possibility of high-tension fuses being accidentally exploded by currents induced in cables, with which they were connected during the occurrence of thunderstorms, or of less violent atmospheric electrical disturbances. Experiments and the results obtained in military service operations, had demonstrated that if insulated wires, immersed in water, buried in the earth, or even extended on the ground, were in sufficient proximity to one another, each cable being in circuit with a high-tension fuse and the earth, the explosion of any of the fuses by a charge from a Leyden jar, or from a dynamo-electric machine of considerable power, might be attended by the simultaneous ignition of fuses attached to adjacent cables, which were not connected with the source of electricity, but which become sufficiently charged by the inductive action of the transmitted current. It therefore appeared very possible that insulated cables extending to land or submarine mines, in which high-tension fuses were enclosed, might become charged inductively during violent atmospheric electrical disturbances to such an extent as to lead to the acciden-

tal explosion of mines with which they were connected. In a report by von Ebner on the defence of Venice, Pola, and Lissa, by submarine mines, in 1866, he refers to the accidental explosion of one of a group of sixteen mines during a heavy thunderstorm, as well as to the explosion of some mines, by the direct charging of the cables, through the firing station having been struck by lightning. Two instances of the accidental explosion of tension fuses by the direct charging of overhead wires during lightning discharges occurred in 1873 at Woolwich. Subsequently an electric cable was laid out at Woolwich along the river bank below low-water mark, and a tension fuse was attached to one extremity, the other being buried. About eleven months afterwards the fuse was exploded by a charge induced in the conductor during a very heavy thunderstorm. In consequence of such difficulties as these experienced in the special application of the high-tension fuses to submarine purposes, the production of comparatively sensitive low-tension fuses, of much greater uniformity of resistance than those employed in former years was made the subject of an elaborate experimental investigation by the lecturer. Different samples of comparatively thin wires, made from commercial platinum, showed very great variations in electrical conductivity. Very considerable differences in the amount of forging to which the metal, in the form of sponge had been subjected, did not importantly affect either its specific gravity or its conductivity, and the fused metal had only a very slightly higher degree of conductivity than the same metal forged from the sponge. The conductivity of very fine wires could therefore be but slightly affected by physical differences in the metal, and the considerable differences in conductivity observed in different samples of platinum were therefore chiefly ascribable to variations in the degree of its purity. It appeared likely that definite alloys might furnish more uniform results than commercial platinum; experiments were therefore made with fine wires of German silver, and of the alloy of sixty-six of silver with thirty-three of platinum employed by Matthiessen for the reproduction of B. A. standards of electrical resistance. Both were greatly superior to ordinary platinum in regard to the resistance opposed to the passage of a current; German silver was in its turn superior to the platinum silver alloy: although the difference was only trifling in the small lengths of fine wire used in a fuse—0.25 in.—while the comparatively ready fusibility of the platinum silver wire contributed, with other physical peculiarities of the two alloys, to reduce the fine German silver wire to about a level with it. Moreover, the latter did not resist the tendency to corrosive action exhibited by gunpowder, and other more readily explosive agents, which had to be placed in close contact with the wire bridge in the construction of a fuse, while the platinum silver was found to remain unaltered under corresponding conditions. Experiments have also been made with alloys of platinum with definite proportions of irridium, the metal with which it is chiefly associated, very fine wires of an alloy containing 10 per cent. of irridium were eventually selected as decidedly the best materials for the production of wire fuses of comparatively high resistance and uniformity, this alloy being found decidedly superior in the latter respect, as well as in point of strength—and therefore of manageableness in the state of very fine wire, 0.001 in. in diameter—to the platinum silver wire. The fuses now used in military and submarine services were made with bridges of iridio platinum wire, containing 10 per cent. of the first-named metal. The electrical gun tubes in the navy were fired by means of a specially arranged Leclanche battery, and branch circuits worked to the different guns; in broadside firing, it was important that the wire bridge of any one of the gun tubes which was first fired should be instantaneously fused on the passage of the current, so as to cut this branch out of circuit; in this respect the comparatively fusible platinum silver alloy appeared to present an advantage, hence the naval electrical fuses were made with bridges of that alloy. Uniformity of electrical resistance had become a matter of such high importance in the delicate arrangements connected with the system of submarine mines, as now perfected, that the very greatest care was bestowed upon the manufacture of service electric fuses and detonators, which were in fact

made, in all their details, with almost the precision bestowed upon delicate scientific instruments, and the successful production of which involved an attention to minutiæ which would surprise a superficial observer.

One of the earliest applications of electricity to the explosion of gunpowder was the firing of guns upon proof at Woolwich by means of a Grove battery and a gun tube, which was fired by a platinum wire bridge, a shunt arrangement being used for directing the current successively into the distinct circuits connected with the guns to be proved. When the high-tension fuse had been devised, gun tubes were made to which it was applied, and an exploder was arranged by Wheatstone, having a large number of shunts, so that as many as twenty-four guns might be brought into connection with the instrument, and successively fired by the depression of separate keys connected with each. The firing of cannon as time signals, was an ancient practice in garrison towns, but the regulation of the time of firing the gun by electrical agency from a distance appears first to have been accomplished in Edinburgh, where, since 1861, the time gun had been fired by a mechanical arrangement, actuated by a clock, the time of which is controlled electrically by the mean time clock at the Royal Observatory on Calton Hill.

Shortly after the establishment of the Edinburgh time gun, others were introduced at Newcastle, Sunderland, Shields, Glasgow, and Greenock. The firing of the gun was arranged for in various ways: in some instances it was effected either direct from the observatory at Edinburgh, or from shorter distances, by means of Wheatstone's magneto-electric exploders. At present there were time-guns at West Hartlepool, Swansea, Tynemouth, Kendal, and Aldershot, which were fired electrically, either by currents direct from London, or by local batteries, which were thrown into circuit at the right moment by means of relays, controlled from St. Martin's le-Grand. About thirteen years ago, the electrical firing of guns, especially from broadsides, was first introdnced into the Navy, with the employment of the Abel high-tension gun tube and voltaic piles. The gun tubes then used were manufactured simply for the proof of cannon and for experimental artillery operations, and were of very simple and cheap construction. Experience proved them to be unfitted to withstand exposure to the very various climatic influences which they had to encounter in her Majesty's ships, and in store in different parts of the world. The low-tension gun tubes, having a bridge of very fine platinum silver wire, surrounded by readily ignitable priming composition, was therefore adopted as much more suitable for our naval requirements. The arrangements for broadsides or independent firing, and also for the firing of guns in turret ships, had been very carefully and successfully elaborated in every detail, including the provision of a so-called drill or dummy electrical gun tube, which was used for practice and refitted by well instructed sailors. The firing keys and all other arrangements connected with electrical gun firing, were specially designed to ensure safety and efficiency at the right moment. The electric detonators for firing outrigger torpedoes, or for other operations to be performed from open boats, corresponded, so far as the bridge was concerned, with the naval electric gun tubes, and were fired with a specially fitted Leclanche battery. These electric appliances were now distributed throughout the Navy, and the men were kept, by instruction and periodical practice, well versed in their use. The application of electricity to the explosion of submarine mines, for purposes of defence and attack, received some attention from the Russians during the Crimean War under the direction of Jacobi; thus a torpedo, arranged to be exploded electrically when coming into collision with a vessel, was discovered at Yeni-Kale during the Kertsch expedition in 1855. Some arrangements were made by the British, at the conclusion of the war, to apply electricity to the explosion of large powder charges for the removal of sunken ships, &c., in Sebastopol and Cronstadt Harbors. In 1859 a system of submarine mines, to be fired through the agency of electricity by operators on shore, was arranged by Von Ebner for the defence of Venice, which, however, never came into practical operation. Early in 1860 Henley's large magneto-electric machine, with a supply of Abel fuses, and stout india-rubber bags, with fittings to resist water-pressure, were

despatched to China, for use in the Peiho river, but no application appeared to have been made of them. The subject of the utilization of electricity for purposes of defence, however, did not receive systematic investigation in England or other countries until some years afterwards, when the great importance of submarine mines, as engines of war, was demonstrated by the number of ships destroyed and injured during the war in America. The application of electricity to the explosion of submarine mines was very limited during that war, but arrangements for its extensive employment were far advanced in the hands of both the Federals and Confederates at the close of the war, men of very high qualifications, such as Captain Maury, Mr. N. J. Holmes and Captain McEvoy having worked arduously and successfully at the subject. The explosion of submerged powder charges by mechanical contrivances, either of self-acting nature or to be set into action at desired periods, was accomplished as far back as 1583, during the siege at Antwerp, by the Duke of Parma, and from that period to 1854, mechanical devices of more or less ingenious and practicable character had been from time to time applied, to some small extent, in different countries, for the explosion of torpedoes. The Russians were the first to apply self-acting mechanical torpedoes with any prospect of success, and had the machines used for the defence of the Baltic been of larger size—they only contained 8lb. or 9lb. of gunpowder—their presence would probably have proved very disastrous to some of the English ships which came into collision with and exploded them. Various mechanical devices for effecting the explosion of torpedoes by their collision with a ship were employed by the Americans, a few of which proved very effective. But although in point of simplicity and cost, a system of defence by means of mechanical torpedoes possessed decided advantages over any extensive arrangements for exploding submarine mines by electric agency, their employment was attended by such considerable risk of accident to those at whose hands they received application that under any circumstances which were likely to occur, they became almost as great a source of danger to friend as to foe. The most important advantage secured by the application of electricity as an exploding agent of submarine mines were as follows:—They might be placed in position with absolute safety to the operators, and rendered active or passive at any moment from the shore; the waters which they were employed to defend were, therefore, never closed to friendly vessels until immediately before the approach of an enemy; they could be fixed at any depth beneath the surface —while mechanical torpedoes must be situated directly or nearly in the path of a passing ship —and they might be removed with as much safety as attended their application.

There were two distinct systems of applying electricity to the explosion of submarine mines. The most simple was that in which the explosion was made dependent upon the completion of the electric circuit by operators stationed at one or more posts of observation on shore; such a system depended, however, for efficiency, on the experience, harmonious action, and constant vigilance of the operators at the exploding—and observing—stations, and was, moreover, entirely useless at night, and in any but clear weather. The other, which might also be used in conjunction with the foregoing, was that of self-acting mines, exploded either by collision with the ship, whereby circuit was completed through the enclosed fuse, or by the vessel striking a circuit closer, whereupon either the mine, moored at some depth beneath, was at once fired, or the necessary signal was given to the operator on shore. Continental nations had followed in our steps in providing themselves with equipments for defensive purposes by submarine mines, and the Danes, Swedes, and Norwegians had pursued the subject of submarine mines, with special activity and success. In the United States the subject of utilization of electricity as an exploding agent for war purposes was being actively pursued, and important improvements in exploding instruments, electric fuses, and other appliances had been made by Smith, Farmer, Hill, Striedinger, and others already mentioned, while no individual had contributed more importantly to the development of the service of submarine explosions than General Abbot, of the United States Engineers.

Illustrations of actual results capable

of being produced in warfare by submarine operations had hitherto been very few; but of the moral effects of submarine mines there had already been abundant illustrations. In the war carried on for six years by the Empire of Brazil and the Republic of Uruguay and the Argentine Republic of Paraguay, the latter managed, by means of submarine mines, to keep at bay, for the whole period, the Brazilian fleet of fifteen ironclads and sixty other men-of-war. In the Russo-Turkish war submarine mines and torpedoes were a source of continued apprehension; and the French naval superiority was paralyzed during the Franco-German war by the existence, or reputed existence, of mines in the Elbe. The application of electricity to the explosion of military mines, and to the demolition of works and buildings, had been of great importance in recent wars in expediting and facilitating the work of the military engineer. The rapidity with which guns, carriages, &c., were disabled and destroyed by a small party of men, who landed after the silencing of the forts at Alexandria, illustrated the advantages of electric exploding arrangements, combined with the great facility afforded for rapid operations by the power possessed of developing the most violent action of gun-cotton, dynamite, &c., through the agency of a detonator. The application of electricity to the explosion of mines for land defences during active war was not an easy operation, inasmuch as not only the preparation of the mines, but also the concealment of electric cables and all appliances from the enemy entailed great difficulties, unless the necessary arrangements could be made in ample time to prevent a knowledge of them reaching the enemy. But few words need be said to recall to the minds of civil engineers the facilities which the employment of electricity to explosive purposes afforded for expediting the carrying out of many kinds of works in which they were immediately interested. Electrical blasting, especially in combination with rock-boring machines, had revolutionized the operation of tunnelling and driving of galleries; and, although in ordinary mining and quarrying operations the additional cost involved in the employment of fuses, conductors, and the exploding machine, was not unfrequently a serious consideration, there were, even in those directions, many occasions when the power of firing a number of shots simultaneously was of great importance. There was little doubt, moreover, that accidents in mining and quarrying would be considerably reduced in number if electrical blasting were more frequently employed. The conveniences presented by electrical firing arrangements, under special circumstances, were interestingly illustrated by a novel proceeding at the launch of a large screw steamer at Kinghorn, in Scotland, which was recently accomplished by placing small charges of dynamite in the wedge-blocks along the sides of the keel and exploding them in pairs, hydraulic power being applied at the moment that the last wedge was shot away. In the deepening of harbors and rivers, and in the removal of natural and artificial submerged obstructions, the advantages of electric firing were so obvious that extended reference to them was unnecessary. A substitute for electrical firing, which had been applied with success to the practically simultaneous firing of several charges, consisted of a simple modification of the Bickford fuse, which, instead of burning slowly, flashed rapidly into flame throughout its length, and hence had received the name of instantaneous fuse, or lightning fuse. The fuse burned at the rate of about 100ft. per second; it had the general appearance of the ordinary mining fuse, but was distinguished from the latter by a colored external coating. Numerous lengths of this fuse were readily coupled up together, so as to form branches leading to different shot-holes, which might be ignited together, so as to fire the holes almost simultaneously. In the navy this fuse was used as a means of firing small gun-cotton charges to be thrown by hand into boats when these engaged each other, the fuse being fired from the attacking boat by means of a small pistol, into the barrel of which the extremity was inserted.

On the railways of England and Wales there were in 1881 2,263 inhabitants per locomotive, as against 2,607 in 1871, and there were 1,017 inhabitants per passenger vehicle as compared with 1,232 in 1861.

THE SECRETION OF GAS IN STEEL CASTINGS.

A Correction by DR. F. C. G. MÜLLER.

From "Iron."

Reserving to myself to deal thoroughly with the importance of silicon in the metallurgy of iron in a paper shortly to be published, I should like to rectify in the following brief reply, a few misunderstandings and errors contained in the open letter addressed to me by M. Pourcel. That letter merely criticises a few minor points in the introductory part of my paper, on the secretion of gas in steel castings. Not the slightest reference is made to the chief argument of the introduction, the large secretion of gas in completely decarbonized iron. I may here add the fact that very often also pig-iron—and not merely the white description—coming direct from tha blast furnace or the cupola, gives castings full of blowholes. In either case we have to deal only with the occlusion of previously absorbed gases, and thus it seems incomprehensible why for the intermediate products, the much more complicated reaction theory should take the place of the absorption theory. But, as already remarked, all these arguments are only subsidiary. The gist of the matter is the absence of carbonic oxide in the blowholes, and the presence of hydrogen and nitrogen.

Now, I have to note the fact that M. Pourcel does not at all doubt the correctness of my experiments, and is continually speaking of escaping hydrogen. He has only desired to disclose "anomalies" in my theory. On his account, therefore, I have not again concisely arranged the whole of my experimental material, but only for those who, like Mr. Snelus and Mr. Richards, think that my borer has decomposed the surrounding water. Besides, my paper is not a polemical treatise; it only contains a few polemical theses, in which I defend myself against direct attacks. Opposed to M. Pourcel, I have always been the attacked party. Once before, two years ago, I was compelled to face him and his followers. But that matter is buried. It is not correct, as M. Pourcel stated at Vienna, that I ill-treated his theory. Up to that time I had not referred to his theory, even with a single word; in fact, I did not know at all that he had a special theory. In my previous paper, I only opposed the statement of M. Gautier, according to which

$$2\,CO + Si = SiO_2 + 2C.$$

In the open letter I am addressed as follows: "No, Dr. Muller. Silicon does not increase the solubility of the gas in the steel; just the contrary." "You know that MM. Troost and Hautefeuille have proved by experiment that silicon almost annuls the solubility of hydrogen in steel." I reply that MM. Troost and Hautefeuille have stated in this connection only the fact that manganiferous iron melted in a hydrogen atmosphere violently scatters in solidifying, whilst silicious iron does not do that. It follows from this that liquid manganiferous iron absorbs more gas at a higher temperature than it can retain at its melting point, whilst silicious iron, on the contrary, possesses no greater degree of absorptive power at a higher temperature than in solidifying. This fact is quite irrelevant to my theory. The question is simply whether the solidified metal, with a greater contents of silicon is able to retain more hydrogen. The other experiments of the enquirers named prove that this is the case. Thus 500 grammes of pig-iron absorbed 46.6, cast steel 7.8, malleable iron 13.9, cubic centimeters of hydrogen. Quite recently, Professor Ledebur has likewise proved by careful experiments, the existence in ferromanganese as well as in silicide, of considerable, and, in both alloys, of equal quantities of hydrogen. It may be added, incidentally, that this eminent enquirer—he is a metallurgist, and not like M. Pourcel's friend, merely a chemist—has given expression to the same view at which I arrived at the same time and quite independently. But besides these experiments, we have also a *demonstratio ad oculos* direct from blast furnace practice, namely, the experiment of M. Pourcel. "The metal to which silicide of

manganese has been added does not give off any carbonic oxide at the moment of solidification, but emits flames of hydrogen, which burn on the surface at the time of casting. . . But if the silicon is added in the form of silicide of iron, and the steel contains only traces of manganese there is no disengagement whatever of gas." While we do not at present enquire into the part played by manganese, we do not lose sight of the following. The steel in question contains hydrogen but of this hydrogen not a trace is secreted as soon as silicon is added. Thus the solubility of hydrogen in silicious iron is demonstrated beautifully and unmistakably by the experiment described.

My remark that in the Bessemer operation violent spiegel reaction leads to solid steel, while slight reaction, notwithstanding high percentage of silicon, gives porous ignots, M. Pourcel has entirely misunderstood. I mean charges which have not been blown enough, and on that account give no or very slight spiegel reaction. The slag was quite liquid in all the charges examined by me, as could not be otherwise, considering the large contents of magnanese in the charge. The fact stated by me and others is no other than that, if the process is interrupted before the bath becomes highly oxygenated, rising steel remains after the addition of spiegeleisen also if there is a large remnant of silicon; but if blowing is continued for some time longer, so that large formation of oxide and strong reaction follow, the same bath gives solid steel. I explain this solidifying effect of a strong spiegel reaction by the supposition that the violent intermolecular development of carbonic oxide carries off the solved hydrogen and nitrogen mechanically, and have confirmed this assumption by gas analysis.

With regard to the participation of the several elements in the disoxidation towards the close of the Bessemer and Martin process, I have stated upon the basis of my experiences that silicon plays a very subordinate part in it. Now, M. Pourcel gives more than a thousand analyses as against my two, in order to show that in adding silicide one-third of Si is consumed. I have examined at different German Bessemer works six spiegel reactions, four of which showed no perceptible decrease of silicon, and two an increase. Those observations confirm what might have been predicted *á priori*, that in the extraordinarily high temperature at the close of the German Bessemer process the oxygen of the bath attacks principally carbon or manganese. Nevertheless, I admit that a little silicon also oxidizes, but that this loss is compensated by a corresponding reduction of silicon from the lining, or even exceeded. In the Martin process I have examined myself one reaction, and received reports respecting two others. There also was no consumption of silicon. But I must correct myself on this point after looking more carefully through my notes. The disoxidation was carried out by first adding ferro-manganese, and next, after this had operated, silicide of iron. Thus, ferro-manganese probably effected the whole reaction. A sample taken after the addition of ferro-manganese showed still a few blowholes; only after further addition of silicide the sample ingots were perfectly solid.

But I should like to remark, respecting the thousand analyses at the Martin works of Terrenoire, that it is not possible in the Martin process to study the spiegel reaction entirely by itself. For the fining process is continued from the slag, by which the added elements must be eliminated, and, in case the bath is not considerably overheated, a large amount of silicon will be consumed. It is, consequently, not known afterwards how much has been used in the disoxidation process proper. I hope to be able in a future paper to report on decisive results in this respect. In the first place at the close of the Thomas process as much as possible of the slag is to be removed and the disoxidation effected by the addition of materials containing various percentages of silicon. Thus a reaction at a high temperature is obtained without disturbing accessory processes. In order to be able to follow the analogous process at a low temperature, the same metal in the oxidized state is poured into a mould, into which by degrees cold silicide or other additions are thrown. But it must not be forgotten that the study of the disoxidation reaction is an independent matter, which decides nothing in the "absorption or reaction" controversy. Whether silicon takes part or not, at any rate a large quantity remains in the

steel, and this enables it to retain a large portion of hydrogen, by which the formation of blowholes is prevented.

Far less than the disoxidation process, the incidents in the middle of the Martin process threw no light upon the point in question. There carbonic oxide is developed regularly, and it is quite unnecessary when M. Pourcel tells me that the development of gas is decreased when silicide is added. Every scientific metallurgist knows that. Even in the ladle the formation of carbonic oxide may proceed further through the better mixing which takes place. But I think that, even if the wildest steel has become quiet after standing for some time in the ladle, reaction has completely ceased. It is too much against all experience that two easily combining liquids which have been poured into each other and stirred were not to give a perfect mixture if the whole is afterwards poured from some height in a powerful stream into a collecting vessel.

Most of my boring experiments were made in ingots of a quietly rising steel rich in silicon and manganese. I mention here that it appeared absolutely quiet on being poured into the ladle. It remained perfectly quiet for about ten seconds also in the mould, and began then to rise slowly without any signs of scattering. The gas secretion forming the worm-like tubules takes place consequently in the already solidified metal after a lengthened interval. It is impossible that it could be caused by a freshly beginning spiegel reaction. Only he who is devoid of any chemical knowledge could resist that conviction. But we may leave entirely out of consideration whether the freshly recurring formation of carbonic oxide is theoretically possible or not. For the blowholes contain no carbonic oxide. They contain hydrogen, as well as from 10 to 20 per cent. of nitrogen. Besides, it is just hydrogen which can never be produced by an oxidizing reaction.

M. Pourcel points to Professor Wedding as an adherent to his theory, and reproaches me for not having mentioned him. Beyond my principal opponents, I have named no one, not even Professor Bauerman, the only one who espoused the cause of hydrogen at Vienna. Up to the present time I am still in doubt to which party Dr. Wedding belongs. No doubt he says that certain synthetical experiments are in favor of M. Pourcel. "It is clear," he states, "that as long as silicon is added to nearly pure iron, all the carbonic oxide, whether soluble or combined with the iron, is reduced to carbon, which unites with the iron, and the blowholes cannot, therefore, contain any more carbonic oxide." This conclusion, consequently, as a matter of fact leads to what I maintain. The blowholes contain, actually, no carbonic oxide, but hydrogen and nitrogen, and do not, therefore, owe their origin to a reaction. As long as the first ingot is not found the pores of which, against the rule, contain chiefly carbonic oxide, it appears indifferent to me whether the not present carbonic oxide might be present if this or that hypothetical reaction took place in the steel or not.

In his well-known work, Dr. Wedding admits both reaction and absorption. With regard to Bessemer steel it is stated by him on page 394:—"A second peculiarity of the Bessemer products consists in the gases which are mechanically dissolved in it, or are formed by chemical reaction after manufacture, and which, it is true, partly escape during solidification, but which also partly remain and form holes." With regard to Martin steel, it is stated on page 549:—"Its property of being more easily applicable to figured castings than Bessemer steel, is based simply upon its greater freedom from absorbed gases."

The Berlin syntheses mentioned are said to have illustrated the reciprocal effect expressed in the following equation:—

$$Si(Fe)_x + 2CO = C_2Fe_x + SiO_2.$$

This reaction, from a chemical point of view, is not at at all improbable, and was discovered years ago by Caron. But it should not be lost sight of that it can take place only at a proportionately low temperature, a red-heat. At the melting point of steel, however, owing to the very greatly increased affinity of carbon to oxygen, the reverse takes place. Such reciprocal reactions are known in chemistry in great numbers. For instance, potassium reduces at a low temperature carbonic acid to carbon, while at a white heat, carbon reduces potassic oxide. In

accordance therewith, in our case the reaction would be:—

$$SiO_2 + C_2Fe_x = 2CO + SiFe_x$$

This, in the first place brings silicon into iron within the blast furnace. In the acid, Bessemer as well as in the Martin process, it likewise effects absorption of silicon and formation of carbonic oxide. It is M. Pourcel who attributes to this reaction a special importance, and cites in his letter a series of fresh proofs. Consequently, the above reciprocal reaction mentioned by Professor Wedding does not take place at the melting point of steel, and M. Pourcel at Vienna, guarded himself most expressly against having ever affirmed it. Even the best syntheses executed in the laboratory should be utilized for the metallurgy of iron only with the greatest care, because the temperatures at the experiments do not as a rule approach the degree reached in the blast-furnace and the manufacture of ingot steel and ingot iron.

THE CAPACITY OF STORAGE RESERVOIRS FOR WATER SUPPLY.

By W. RIPPL, Royal Technical High School at Gratz, Styria.

Selected Papers of the Institution of Civil Engineers.

In the English system for the water supply of towns, by collecting the drainage of large catchment basins, one of the most important problems is the determination of the capacity for storage, which should be provided in the reservoirs.

In the earlier works designed on this plan this point did not receive sufficient attention, because at that time the data required were not available. Hence reservoirs were constructed of insufficient size, causing a sensible deficiency in the water-supply in dry seasons. As the dams of the storage reservoirs could not be raised in height without endangering their stability, new reservoirs and new gathering grounds had to be added—a proceeding sometimes difficult and always costly.

For a long time engineers were obliged to apply the results of experience gained in existing waterworks to the design of new systems, by giving to the reservoirs a fixed capacity for a given area of gathering ground. If, for example, in an existing system of water-supply, a storage capacity of 2,500 cubic meters (88,288 cubic feet) was found adequate for 1,000 hectares (2,471 acres) of gathering ground, the reservoir of a new system was designed to afford a proportionate storage capacity. But as the amount of storage necessary depends on circumstances which vary in different localities, it is clear that in reservoirs thus designed, it is only by accident that a deficiency of water-supply, in a series of years is prevented.

2. Hawksley's Formula.

The purpose of the storage reservoir is to equalize the fluctuations of supply and demand during an indefinitely long period of time. The circumstances of an average year are therefore not sufficient to determine the quantity to be stored. Hence Mr. Hawksley has given an empirical rule, based on the conditions which obtain during a period of three consecutive dry years, or years in which the rainfall is below the average.*

Let R be the average rainfall during three consecutive dry years estimated in millimeters, Z the number of days' storage which should be provided.

Then, according to Mr. Hawksley,†

$$Z = \frac{1{,}000}{0.198\sqrt{R}}.$$

The volume of water to be stored in one day is

$$T = B + C + V - D,$$

---

* In such periods the average annual rainfall is taken as one-sixth less than the average rainfall of a long series of years.

† If R is in inches

$$Z = \frac{1{,}000}{\sqrt{R}}.$$

where B=demand of the town;
C=compensation to the stream;
V=loss by evaporation from the surface of the reservoir;
D=dry-weather flow into the reservoir.

The quantities are all estimated for a period of twenty-four hours.

According to Mr. Hawksley's formula, and in England,

$$Z = 100 \text{ to } 250 \text{ days.}$$

The formula is suitable only for English conditions, where the amount of compensation-water is regulated by law, being usually one-third to one-fourth of the available supply from the catchment basin. The formula would not be applicable for German or Austrian localities, where the amount of compensation-water is settled by free agreement with the owners of the water rights.

3. Empirical Method hitherto Adopted.

The method most commonly adopted in deciding the capacity of a storage reservoir is a purely empirical one, and depends on the consideration of the period of greatest drought only.

Any probable quantity is assumed for the capacity of the storage reservoir, and it is further assumed that the reservoir is full at the beginning of the period of drought. By simple addition of the monthly supply to the reservoir during such a period, and subtraction of the supply to the town, and for compensation, also estimated for successive months, a calculation is made of the quantity in the reservoir at the end of each month for a period of a year. Should the calculation show a deficiency (the volume in the reservoir appearing as a negative quantity), the capacity originally assumed for the reservoir is increased, and the caculation is repeated.

The proceeding is an imperfect one, and is also laborious. The calculation may be shortened by assuming, as the capacity of the reservoir, the sum of all the deficiencies during the drought instead of any imperical quantity, and then making the detailed calculation for the period of a year.

But this method of calculation is open to the objection that it is only in certain cases that the capacity of the reservoir arrived at is sufficient to equalize the fluctuation of supply and demand, not only during a single drought, but during a series of periods in which the supply is deficient.

The records of the rainfall at Vienna prove this assertion. The least rainfall, and consequently the least available supply to the reservoir, generally occurs in the winter months. Thus for December, January and February, the standard rainfall is at the rate of 111 millimeters (4.44 inches). The driest winter was that of 1857-58, when for these months the rainfall was 42 millimeters (1.68 inch). In calculating the capacity of the reservoirs for the water-supply to West Vienna, this winter was taken as the basis of the calculation by the technical experts, and the empirical caculation for the driest year (1858) was made by the method explained above.

But the graphical method of the author applied to this case, and embracing a period of thirty-seven years, during which records of rainfall were available, showed that both in the dry period, 1858–59, and in that of 1855–56, a greater storage capacity was required than in the period 1857–58. Further, the true measure of the storage required was found to be that necessary to equalize the supply and demand during the period extending from May 1855 to May 1856, as it was during that period that the greatest deficiency for the whole period of thirty-seven years was found to occur.

4. New Graphical Method of Calculation.

The following is an outline of the author's method of determining the capacity required for storage, to equalize the supply and demand during any period for which rainfall observations are available.

First the supply to the reservoir and the outflow are estimated for successive equal periods of time, usually one month, and for the whole period of time to be considered. The successive intervals of time are set off along an axis of abscissas, to any convenient scale, and the estimated inflow to and outflow from the reservoir in each interval are set up as ordinates. Connecting the points thus found two curves (or broken lines) are obtained,

which may be denominated briefly as the supply-curve and the demand-curve.

The form of the supply curve *a a*, Fig. 1, or curve, the ordinates of which represent the available supply to the reservoir, will in general be similar to the rainfall-curve.

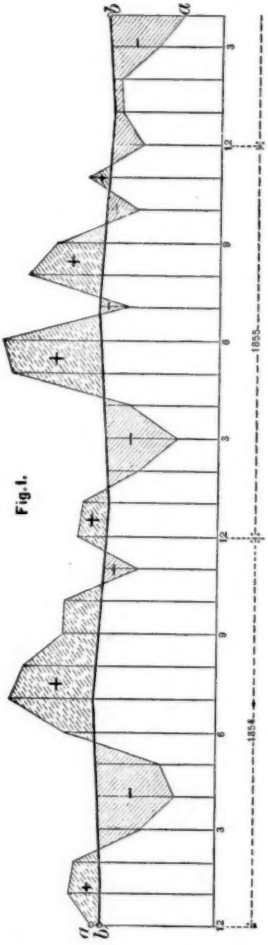


Fig. 1.

The demand-curve will have a form similar to the curve *b b*. It will be the same every year, if the compensation-water is taken entirely from the reservoir. If, however, the compensation-water is partly supplied from streams which do not flow into the reservoir, as in the case of the West Vienna project, then the quantity of compensation-water from the reservoir, and consequently the whole outflow from the reservoir, will vary, increasing as the rainfall decreases, and *vice versa*.

The difference between the two ordinates at each month's end represents either a surplus (positive) or deficiency (negative), according as the ordinate of the supply-curve is greater or less than the ordinate of the demand-curve. These surpluses and deficiencies for each month

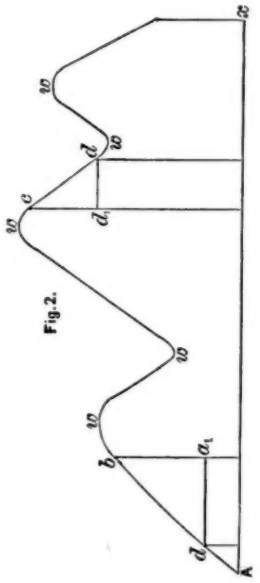


Fig. 2.

are measured and entered in a table as follows:

Column 1 gives the periods for which the successive surpluses or deficiencies are estimated.

Column 2 the surpluses or positive differences of the ordinates.

Column 3 the deficiencies or negative differences of the ordinates.

Column 4 contains, opposite each date, the algebraic sum of the numbers in columns 2 and 3 up to that date.

The following table gives the numbers thus obtained from the diagram, Fig. 1.

Next, the numbers in column 4 are set off on a diagram as ordinates, the abscissas being the intervals of time as before (Fig. 2). The foot point of each

ordinate is the end of the corresponding abscissa, representing the period of time estimated from the point of time at which the calculations begin. By joining the ends of the ordinates a curve is obtained, which will be denominated the mass-curve.

	1	2	3	4
		Differences.		Algebraic sum or ordinate of mass-curve.
		Surplus. +	Defi'cy. −	
1854	End of Dec.	—	—	0
1855	" Jan.	1,048,877	—	+1,048,877
"	" Feb.	697,030	—	1,745,907
"	" Mar.	—	989,153	756,754
"	" Ap'l	—	671,962	84,792
"	" May	5,330,640	—	4,415,432
"	" Jun.	5,006,492	—	9,421,924
"	" July	—	197,561	9,224,363
"	" Aug.	4,246,729	—	13,471,092
"	" Sep.	1,426,470	—	14,897,562
"	" Oct.	—	377,643	14,519,919
"	" Nov.	1,621,640	—	16,141,559
"	" Dec.	—	397,039	15,744,520
1856	" Jan.	828,979	—	16,573,499
"	" Feb.	833,518	—	17,407,017
"	" Mar.	—	1,368,287	16,038,730
"	" Ap'l	—	2,651,043	13,387,687
"	" May	395,225	—	13,702,912
"	" June	1,548,786	—	15,331,698

The mass-curve has the following properties:

1. For the interval of time between any two points on the axis of abscissas, the difference of the corresponding ordinates is the surplus, if positive, or deficiency, if negative, during that interval. An ascending part of the curve therefore marks a period during which the quantity in the reservoir is increasing and a descending part of the curve a period during which the quantity in the reservoir is diminishing.

The crests and hollows, w, of the curve indicate those instants of time at which the supply and demand are equal

2. If a horizontal line is drawn forwards at a crest, for example, the line w_2 f at w_2 (Fig. 3), the distance e f of a point e on the descending part of the curve, from the horizontal line, represents the total deficiency within the period represented by w_2 f. To cover this deficiency there must have been previously an equal storage, and e f therefore represents the amount of storage required to meet the deficiency in the period w_2 f.

3. From what previous point of time the storage to meet the deficiency must have commenced is found, by drawing the horizontal line e g, backwards from e, till it meets an ascending part of the curve. In the period represented by g e the supply and demand are equal. Hence g e may be termed a balancing line. This is true of any point such as x, Fig 4, on a descending part of the curve, that is the supply and demand are equal for the period represented by the balancing line x y, all the subordinate deficiencies being balanced by corresponding surpluses, as is indicated by the shading of the diagram.

4. In the mass-curve certain hollows t_1, t_2, t_3, (Fig. 5) may be selected. Lines drawn through these points mark off periods within which the surplus during one part of each period must be stored to balance the deficiency in another part of the period.

5. The quantity of water represented by the vertical projections Y_1, Y_2. Y_3,.... of the remaining portions of the ascending curve, between the lines, flows away or at all events is not required to meet the demand during the period of time considered.

6. The vertical distances J_1, J_2, J_3,.... between the lowest and highest crests and hollows in each period are the storage capacities required to equalize the fluctuations of supply and demand during those periods.

7. Balancing quantity J. The greatest possible vertical distance J, between a crest and hollow for any one period represents the capacity of the storage reservoir which is sought for. For if the storage reservoir is capable of equalizing the supply and demand during the period in which J is greatest, it is sufficient in all other periods.

8. The period in which this greatest value of J occurs is therefore the critical period. In the case shown in Fig. 5, the critical period is that represented by t', t_1; during that period all the surplus of supply over demand during parts of the period must be stored to meet the deficiency in the remainder of the period.

9. The ordinates of the mass-curve will be positive (drawn upwards) so long as the sum of the surpluses is greater than

Fig. 4.

Fig. 3.

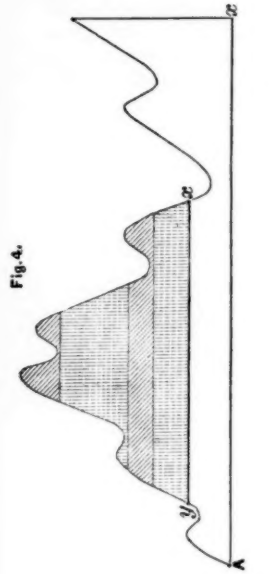


Fig. 4.

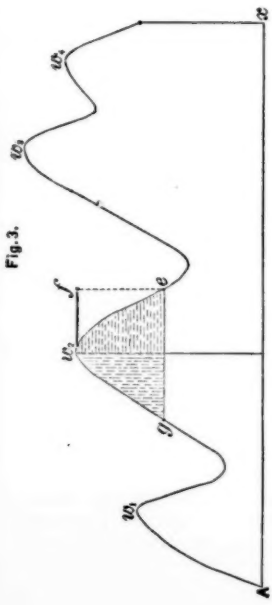


Fig. 3.

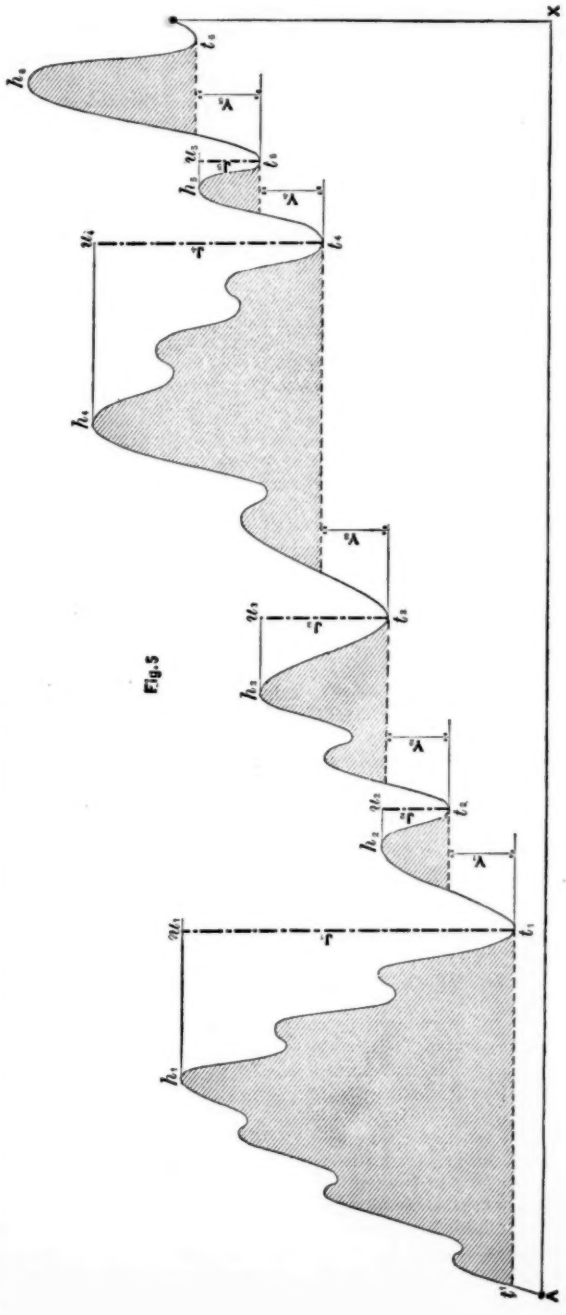
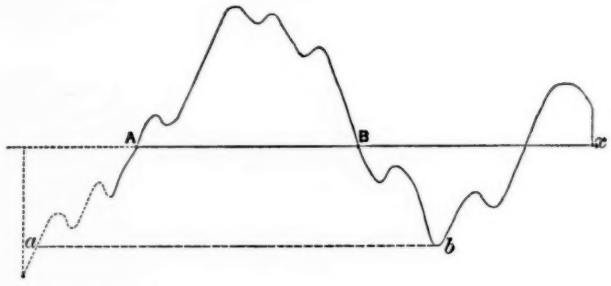


Fig. 5

the sum of the deficiencies up to the point of time considered. When the mass-curve crosses the axis of abscissas the whole previous surplus is exhausted, and if it falls below the axis of abscissas, it is necessary to carry back the curve to some earlier point of time than that at which it was first started. Thus, for example, suppose the curve had been

Fig. 6.



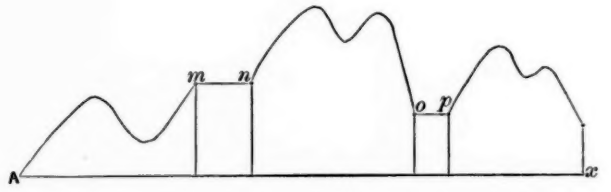
drawn from A (Fig. 6), and had been observed to fall below the axis of abscissas beyond B, then it would be necessary to prolong the curve backwards from A to *a* to make a complete investigation of the period within which the points A and B occur possible, since they fall within a period the balancing line of which is *a b*. In other words the mass-curve will remain above the axis of abscissas so long as the total supply estimated from the beginning of the time considered exceeds the total demand, and only falls below it if the demand exceeds the supply.

10. The mass-curve becomes a straight line parallel to the axis of abscissas for any periods such as *m n*, *o p* (Fig. 7), during which the supply and demand are exactly equal. Such cases, however, occur rarely in practice.

It is clear from what has been said that a single period of drought does not afford a safe basis for determining the proper storage capacity of a reservoir. That storage capacity can only be determined with safety, by examining a series of such periods. Hence also it is not the year in which the least total rainfall occurs which gives the measure of the storage capacity required, but the period in which the greatest fluctuation of supply and demand happens. The limitation of the time considered to a year is erroneous in principle, because the year is in reference to the question to be solved an unessential condition. The essential intervals of time are the periods during which the supply and demand are balanced.

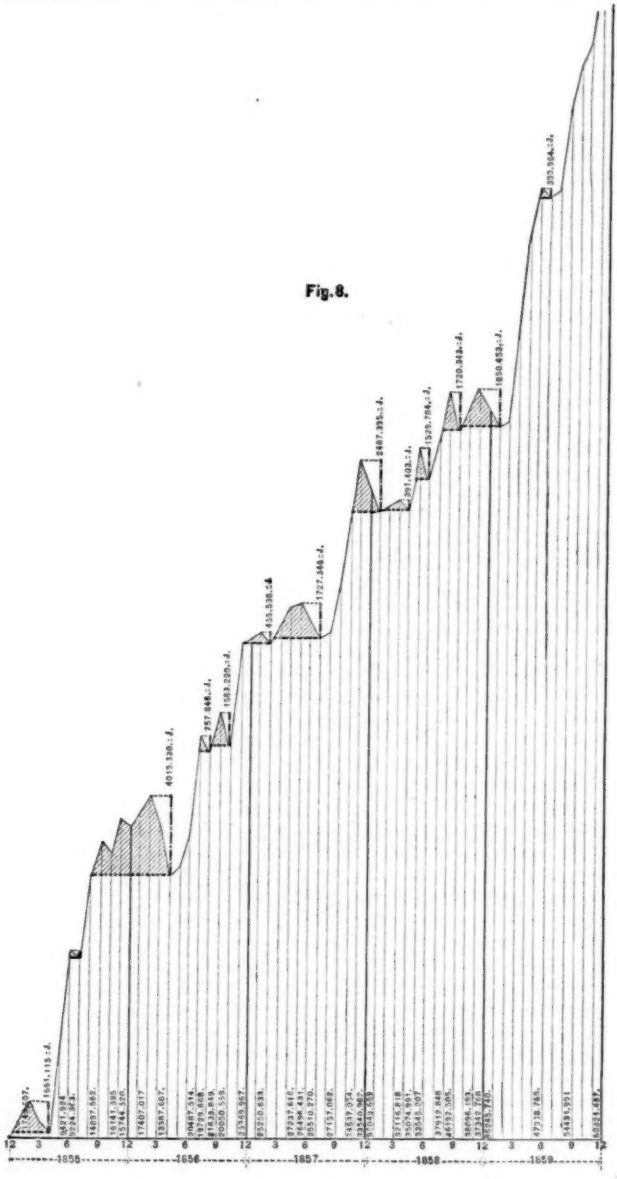
Fig. 8, represents part of a mass-curve

Fig. 7.



based on observations of the rainfall in the district of the Wiener Wald, and comprises those portions of the curve only which required to be taken into detailed consideration. The curve was drawn on the method described above, and by examining it, it will be seen that a storage capacity of 4,019,330 cubic meters is required, to equalize the greatest fluctuation of supply and demand

Fig. 8.



during a period extending from the end of 1854 to the end of 1859. It will be seen also that it is not in the driest year, 1858, that the greatest fluctuation occurred, but in a period extending from the end of August, 1855, to the end of April, 1856. It is this period, therefore, which is the critical period, and which determines the capacity required for storage.

THE MASONRY AND CARPENTRY OF THE GREEKS AND ROMANS.

From "The Builder."

PROFESSOR C. T. NEWTON, C.B., delivered the fourth of a course of eight lectures on the useful and decorative arts of the Greeks and Romans, on Friday, the 11th inst., taking as his subject "Masonry and Carpentry." He observed that in the present lecture he had to deal with two of the most useful arts,—arts which enabled people to obtain shelter from the weather, viz., the arts or handicrafts of masonry and carpentry. Those arts, as he need hardly say, were closely connected with architecture on the one hand, and with purely decorative work on the other. In the first instance he desired to say a word or two as to the condition of artisans generally in Greece, and as to the estimation in which artisans were held. In the poems of Homer the few artisans or skilled workmen who were noticed at all were put rather prominently forward, as if they were highly esteemed in their several crafts. We found mentioned by name several craftsmen, such as workers in metal and workers in wood. The lecturer was not aware, however, that any masons were mentioned by name in the Homeric poems; but Paris was described as building his own house with the assistance of masons, while the walls of Troy were believed by the ancients to have been built by two of the gods, and it was recorded that a disagreement about this work was the original cause of the quarrel between the Trojan dynasty and some of the gods, for it was contended that the two gods who built the walls were not paid their proper wages. We obtained in Homer the idea that the craftsman was a man very necessary to the community, and that he was regarded more or less as a *demiurgos*, or public servant, just as the physician in those days was a public servant who undertook to look after the health of the army. Of course it was to be concluded that in an heroic age such as Homer described the craftsmen would be very few in number, although their services would be very necessary. It was remarkable that Ulysses himself took rank as a carpenter, for Homer described how the king made his own bedstead, which was spoken of as being inlaid with ivory and other ornaments, and supported by the stem of an olive-tree. In tracing the history of the Greeks down from the Homeric age to, say, the time of Pericles, we found in Attic writers of the period a fine distinction drawn between the craftsman and the full citizen, *i.e.*, the citizen who enjoyed the fullest privileges. The full citizen was a member of the Athenian public who was not only endowed with great privileges, but he had to fulfil certain high and imperative duties, such, for instance, as taking part in the defence of his country, and to prepare himself for this particular work by carrying on his gymnastic exercises. On the other hand, the man who was perpetually working at his trade could hardly be said to take any part in public affairs, or to have any forensic life. He stood at his stall and worked at his trade while those who were not obliged to maintain themselves by working as craftsmen took part in the public assembly. Probably in the greater number of cases the craftsmen or skilled workmen were not full citizens. They were either slaves or strangers whom the Greeks allowed to live in Athens and practise their callings, but without giving them the privileges of citizenship. The extensive works which were going on in the Periclean age naturally attracted large numbers of these foreign workmen, including both masons and carpenters. The trades followed by these artificers descended from father to son, but the hereditary transmission of trades was not so exclusive as to involve anything like caste, neither did we find that marked tendency to form corporations or guilds of craftsmen which we saw in Roman times, and which went on throughout the Middle Ages. Proceeding with the immediate subject of his lecture, Professor Newton said that unfortunately there was very little to be found in Greek or

Roman writings as to masons and carpenters. He presumed that the German works which dealt with trades and crafts had brought together every passage from the ancient authors that could throw any light upon the subject. Such passages, however, were very scanty and meagre. There were, however, other sources of information, as in the mouments themselves. From the writings of Homer it was quite clear that there must have been workmen at a comparatively early period, capable of making the structures of polished marble and the furniture, which were described by the poet. That the Homeric descriptions were trustworthy had been proven by the researches of Dr. Schliemann, whose discovery of the Cyclopean walls of Mycenæ and of the neighboring fortress of Tiryns afforded a remarkable confirmation of some of the poet's statements. Cyclopean walls of this character were noticed by Pausanias as being regarded, in his time, as of remote antiquity. They derived their name from the belief of the ancients that they were built by the Cyclopes, a one-eyed race of beings of gigantic stature. These walls were really specimens of military architecture, those at Tiryns being as much as 27 ft. in thickness. Such walls generally contained internal covered galleries within their thickness, with loopholes from which to take aim at an enemy. Pausanias said of the blocks of stone of which the walls of Tiryns were built, that there were none so small as to be conveyed by less than a pair of mules- He described stones 10 ft. and 12 ft. long throughout these walls, and the immense size of the blocks had been urged as proof of the great age of the walls. Modern writers on prehistoric man had invented the word "megalithic," and spoke of the "megalithic period" as that during which structures like Stonehenge and the walls of Tiryns were built. The lecturer thought it very probable that the earliest walls built in Greece were composed of very large stones. But on examining the examples of Cyclopean Pelasgic or other early masonry extant, it would be found that they were of two kinds, first of all, those in which rectangular blocks of stone were used, laid in regular courses, though some what irregularly ;and secondly, those which were of what might be called polygonal masonry which was composed of stones with many sides and with many angles. At Mycenæ both kinds of masonry existed, and in both kinds the enormous size of the blocks was apparent. To convey those huge blocks up mountain sides, and to place them upon their proper beds, must have been a very difficult operation mechanically, especially in those times when, it might be assumed, the art of road-making was not much understood. The large blocks were often found fitted together imperfectly, leaving interstices in which other and smaller blocks were fitted. Walls of this character were regarded by Dr. Schliemann as belonging to the earliest period. After this early period of imperfect joints or interstitial stones, we came to a later masonry, in which the blocks were carefully and solidly fitted together, so that the wall presented a perfect face. With this, however, there was a diminution in the size of the blocks. The polygonal masonry of the Greeks no doubt owed its distinguishing characteristic to the application of the law of cleavage, with which the Greeks must have been familiar. The lecturer said he referred to these points because they had an immediate bearing on the question of the age of the things discovered by Dr. Schliemann at Mycenæ and Tiryns and at Orchomenos in Bœotia. He (Professor Newton) had ventured to ascribe those antiquities to a very early time,—as early as the year 1200 B. C. It might be said that it was only arguing in a circle to say that because at Mycenæ and Tiryns there were specimens of very old masonry, therefore the things found by Dr, Schlieman were old; but the lecturer contended that the opinion of the ancients themselves was worth a good deal, for they had a range of observation over deserted and ruined cities which we never possessed. But those of his hearers who cared to pursue the question would find it very ably discussed from the negative side in an excellent dissertation on the subject in the fifth volume of the "Classical Museum," by Mr. Bunbury. In the later period of masonry the chisel was very skillfully used. In the harbor of Cnidos are the remains of a mole, the blocks of stone composing which, the lecturer believed, from his personal observation, to have been fitted together in the same way as the walls of Mycenæ

and Tiryns. When he (the lecturer) was at Cnidos, he was accompanied by an engineer, who assured him that one block on the top of the mole weighed at least 50 tons. To get such an enormous block into position with the simple mechanical means which the ancients had at command was no small undertaking. They had no hydraulic jacks, no steam power, but only such simple appliances as the pulley and wheel. Coming to the masons' workmanship exhibited in Greek architecture of the best period, the lecturer observed that the perfection of the joints was a marvel to behold, even at the present day. In building up a column composed of a number of blocks of marble or stone, the problem was how to place each drum, weighing perhaps, 2 tons or 3 tons, upon the bed prepared for it, with absolute exactitude; another problem was how to place in position a piece of architrave, say 18ft. long (for such dimensions were adopted) without injuring it at the edge? Such feats were accomplished by the Greeks, and wherever one looked, whether at Athens, at Priene, or at any other of the sites famous for temples or other buildings, the fragments of architecture which remained exhibited the same beautiful perfection of the joints, called by the Greeks *harmos*. On careful examination of these joints it would be found that the outer edges or margins of the surfaces which were to come in contact were polished. These polished margins inclosed the rougher surfaces of the stones, which were sunk a little so as to allow the polished surfaces to come close together. Illustrations of this were to be seen in the British Museum in the marble tiles from the temple of Artemis at Athens. Mr. Penrose, in his valuable work on the Parthenon, referring to the accuracy of the joints, expressed his inability to tell how the builders could have got the joints so perfectly true, but he suggested that a pin or pivot was fixed in the lower of two drums and a hole to receive the pivot made in the lower surface of the uppermost of the two drums, which was made to revolve upon the lower drum, and so grind the surfaces true. But, whatever might be said in favor of that theory, it obviously would not explain how the Greeks managed to get such perfection of jointing in such features as architraves, &c. Since Mr. Penrose's book had been published, an important inscription, in the shape of an order or law giving directions as to the construction of a temple, had been discovered. This inscription was really the architect's specification, and in it the point was very strongly insisted upon all through that every block of stone used for the temple should be prepared and dressed in a particular manner, and be subject to the approval of the surveyor appointed by the city. Constant reference was made in this specification to the polishing of the joints by the application of vermillion and a particular sort of oil to the surfaces of the joints. Although that inscription had been published in Germany by Fabritius, he had not so far as the lecturer knew, proposed any explanation of that combination of oil and vermillion. A very interesting translation of the inscription was published in the *Builder* a few years ago, probably written by Mr. Watkiss Llyod, who was a very ingenious commentator upon Greek architecture. The writer threw out the hint that the vermillion was used for the purpose of ascertaining whether the two surfaces of marble, which were to be contiguous, were perfect planes. The mode of application was this,—taking two drums of columns, which had to fit truly together, if one of the intended contiguous surfaces were covered with some conspicuous wet color, with which the other contiguous surface were brought in contact by placing the stones one above another in their intended position, it would be readily apparent on separating the stones whether there were any inequalities in their contiguous surfaces. A similar process of working was followed, he found, by masons in the present day, so that here was an illustration of a method of work, probably invented by the Greeks, coming down to the present time from generation to generation of masons. The Greek builders used no mortar, but where the work was of the best character they used cramps either of stone or copper dovetailed into the work. The copper so used being of considerable value, there was no doubt that many ancient buildings had been ruined simply for the sake of obtaining the copper cramps. It was difficult to arrive at any conclusion as to the rate of wages paid to Greek masons, inasmuch as the evi-

dence afforded by inscriptions, &c., did not throw any light on the question whether the rates mentioned included food and lodging. Proceeding to speak of Roman masonry, the lecturer alluded to the way in which the Romans used brick and rubble as a backing or filling-in to their masonry. Their mortar was of very great strength, due to the fact that in its composition they used *pozzuolana*, an earth found near Puteoli. The Romans built walls of brick, sometimes laid in regular courses, sometimes in a kind of *opus reticulatum*, contenting themselves with a mere veneering of marble or stone. With regard to the carpenters' work of the Greeks and Romans, there was very little that could be said, for hardly any of their woodwork had come down to us. It was evident, however, from passages in Homer's works, that even in the times of the poet there must have existed some very good specimens of inlaid woodwork, prototypes of that of which we saw representations on the earliest vases and on various ancient monuments. In the Archaic Greek room at the British Museum there were some chairs which were more elegant in form and more honest in construction than some modern chairs, for they were constructed with mortise-and-tenon joints. The Greeks were certainly acquainted with glue from the time of Homer, but, wiser than modern furniture-makers, they preferred to use the mortise and tenon. With regard to the coarser work of the carpenter, such as that required for the roofs of buildings, a good idea of it was to be gained from the roofs of the Etruscan tombs, many of which had been carved in the rock in evident imitation of timber structures.

HARD ARMOR.

From "The Engineer."

For many years the only kinds of armor that found favor in any country consisted of wrought iron in some form, whether laminated, plate upon plate, or solid. Now a change is taking place, the magnitude and importance of which we venture to think is not realized as yet by the authorities of any country. It is taking place gradually, and for this reason, perhaps, its full significance has escaped notice. We believe it will, in a measure, re-shape what has latterly been laid down as to guns and projectiles, and perhaps ships also. If we are right, we are speaking of a question of unusual interest and importance. We will endeavor to be clear and concise. The matter may be briefly put as follows: Until comparatively recently wrought iron armor—which for the sake of distinction we call "soft" armor—was the only kind employed. The method almost universally adopted of attacking it was to perforate it—that is, to drive projectiles through it. Now armor of a harder nature has largely come in—that is, chilled iron shields for coast batteries, and steel and steel-faced armor for ships. This can be made to resist perforation; it must be destroyed by breaking into fragments. Such armor then, for the sake of distinction, may be called "hard" armor. It will not perhaps come in universally, but we think, as time goes on, it will become almost universal, for reasons to be given hereafter. And now comes the important point. The operation of breaking up plates and that of perforating them are of a sufficiently distinct and separate character to be best performed by different natures of guns and different kinds of projectile; and to such an extent is this the case, that we believe it may affect the class of ships employed. This is not yet clearly recognized, as we think we can show by the fact that experiments in breaking up plates are still brought to the standards, or based on the data, which apply to perforation—which, as we have said above, are, we believe, liable to give very wrong results when so used.

The long courses of experiments against soft armor have furnished data which have been employed in various ways by different authorities in drawing up formulæ by which to calculate perforation. In no case has a perfect sys-

tem been drawn up. All require a certain amount of empirical correction, but with this very good results can be obtained on several systems. In each of them the result depends on the amount of energy, or stored-up work, in the shot being divided by either the area or the diameter of the whole made in the plate; the empirical correction varying according to which system is employed. It naturally must follow, both in theory and practice, that a shot of small diameter has a great advantage in only having to make a small hole; hence the remarkable results which have been obtained by new type guns firing projectiles of small diameter at high velocities. In the work of perforation so great is the advantage that may be obtained by this class of guns, that they naturally have found their way into our most important armaments. If a long 18-ton 9.45 in. gun is able to perforate as much armor as one of 38 tons and 12½ in. calibre, it is natural that the former would be preferred, or, at all events, some approach to the type which gave such results will be preferred. This has greatly strengthened the desire for moderately large guns instead of those of extreme size. If a 43-ton gun could be made having a power of perforating 24 in. of armor, it might naturally be preferred to the Inflexible 80-ton guns, which perforate little more than 25 in. Of course, the latter would fire a larger and more formidable shell; still, this advantage would be dearly bought by so enormous an increase of weight. The ships, to a great extent, took their shape on the same general lines. The Italians were, as it were, bid welcome to their monster ships and 100-ton guns, which, after all, could only perforate about 28 in. of armor. The 43-ton guns of the Colossus or Edinburgh ought to pierce their sides at close quarters, and the additional power seemed dearly bought in the heavier guns. Even in the future, perforation was expected to effect great things. Steel shells could be driven clean through thick armor, not only without fracturing, but without setting up materially; so that there was a good prospect of their carrying bursting charges into the interior of armored structures, and thus, in a great measure, destroying the value of armor—for it is naturally held that so long as dead metal only is driven through the side of an armor-clad she maintains her chief advantage over wooden ships.

Before this had taken place, however, success had been achieved in another direction, of such a character as to make it impossible, in our opinion, for these steel shells containing gun-cotton to have any great future importance, and also such as to upset previous calculations as to guns and armor generally. This, perhaps, is not yet recognized; indeed, quite recently we heard one of the very highest authorities complaining that experiments on what appeared to him so important a subject, namely, the carriage of gun-cotton bursting charges through armor by steel shell, were not pushed on. Now we believe that while the action of steel shell containing gun-cotton bursting charges may be very powerful, and well worth investigation, its application to armor will be very limited indeed, owing to the introduction of the hard armor of which we have now to speak. In 1873 Grusen's *chilled iron armor* had so far showed its powers of resistance that its adoption became only a matter of time. It has now taken its place as the only kind of coast armor employed by almost every nation except England. Russia, Germany, France, Austria, Italy, Spain, Portugal, Holland and Belgium, and Denmark, have all in some shape adopted it. This is the hardest kind of armor extant. It is impossible to perforate it. It must be gradually broken up, which can be done with steel projectiles. Then again, in 1876, the first Spezia trials with the 100-ton gun established the power of *steel* to stop the passage of a projectile whose power of perforation would have been far in excess of the plate had the latter behaved like wrought iron. The steel, it is true, effected this result, by transmitting the shock through its mass, and so suffering wholesale destruction. This, however, is preferable to perforation if there is any possibility of the latter taking the form of a steel shell passing intact into the vessel, and then bursting under the force of explosion of an enormously powerful gun-cotton charge. Steel was, at that time, adopted by Italy, and soon after *steel-faced armor* was so far successfully made and perfected in this country that this material is now adopted

for most of the powerful ships built in England or abroad.

Now the conditions of attack are changed. Chilled projectiles have, in this country, been declared almost useless against steel-faced armor, for reasons dwelt on in the report on the Spezia trials of *The Engineer* of December 1st, 1882. More than that, it appears probable that our basis of calculation is no longer right. One shot can hardly claim an advantage over another in the circumstance that it requires a smaller hole to enable it to pass through a plate, if, as a matter of fact, neither of them is able to make any hole at all. Hence it appears probable that the effect of a shot against hard armor is not proportional to its power of perforation, but simply in proportion to its total energy or stored-up work, coupled with its power of holding together, so as to deliver the maximum amount of such work possible before breaking up. It is true that the shot's point penetrates a certain distance into steel, but in most cases of steel-faced armor, and in all cases of hard steel, the plate keeps out the shot, and only yields by breaking up and becoming dislodged. In such cases we believe that the effect is proportional chiefly to the stored-up work; and we believe that certain artillerists, Captain Andrew Noble for one, have long recognized this, yet in every experiment, British or foreign, the authorities seem to match the shot against the armor, whether hard or soft, according to its power of perforation.

The total amount of energy contained in the blows delivered on any shield have been noticed especially by Colonel Inglis in his reports, but no systematic use of such statements has been made as far as we know. This is easily accounted for; for in the case of wrought iron plates, when the iron was not actually perforated, it would bear so large a number of blows that destruction by such means was not generally practicable; now it is otherwise. We have returned to the days of "racking." Hard armor must be destroyed in this manner, and it seems reasonable to think that measuring the effect of a blow on hard armor by its power of perforation into soft armor is grossly wrong. We will give a few examples. On July 21st, 1880—*vide Engineer*, July 30th, 1880—a 12½ in. chilled shot weighing 828 lb. struck an 18 in. steel-faced plate with 1504 ft. velocity. Its striking energy was 12,980 foot-tons; its calculated power of perforation equal to 18.57 in. of iron. The shot broke up against the plate producing some insignificant face cracks and other injuries. At Spezia in November last—*vide Engineer*, November 24th, 1882—chilled shot from the 17.72 in. muzzle-loading 100-ton gun struck steel-faced plates of Cammell and Brown 18.9 in. thick, with velocities of 1219 ft. and 1222 ft. respectively. The shot weighed about 2000 lbs. each, hence the calculated perforations should be 19.26 in. and 19.33 in., the energies being 20,600 and 20,710 foot-tons. The Cammell plate had a large piece separated by a through crack or fracture, and the Brown plate had long cracks, through which the plate gave way on the next round. It is difficult to compare these cases fairly, but it may be assumed that Cammell's plate in 1882 was at least as good as that in 1880, if not better; yet the difference in the fracture in the thicker plate is much greater than that shown by the relative figures 18.57 and 19.26. We have suggested in the report on Spezia that the Italian shot held together better. This it might do, from the metal being softer and more tenacious, as well as from the striking velocity being lower; but, on the other hand, it may well lead us to notice that the shot which produced so little effect had under 13,000 foot-tons, while the others had over 20,000—a fact that we are tempted to forget from the pernicious habit of matching the shot against the plate by giving its perforation through wrought iron as a standard. The energy per ton of metal in the plate might appear to furnish a sort of standard of comparison. This amounts to about 541 foot-tons per ton of metal for the Shoebury 1880 plate, and 654 foot-tons for the first blow at the Cammell plate at Spezia, the latter plate weighing 31.5 tons, and the former about 24 tons probably.

To get the full difference between probable smashing power and penetration, we have to compare a new-type gun with some piece fired at a comparatively low velocity. Take, for example, the 43-ton gun at the muzzle and the 100-ton muzzle-loading gun at 2,200 yards range.

Their perforations will be seen to be about the same, namely, 24 in. The 100-ton gun shot, however, has 31,200 foot-tons energy, while that of the 43-ton gun is only 19,800—that is, in larger proportion than 3 to 2—and this is with a velocity of 1,500 ft. against 2,000 ft.; from which it probably follows that the larger shot will break up less, and therefore impress more of its energy on the plate than the smaller one. Possibly, then, the smashing effect of the big shot will be nearly double that of the less one. Yet, if the official diagrams or rules be taken, the perforation is seen to be the same, and no other method for estimating the probable effect of shot is generally suggested. And this brings us on to the question of the best gun for perforation through soft armor and for racking.

Against wrought iron—that is, soft armor—as we have said, the new-type guns perform wonderfully well. There, for example, is the 43-ton gun which at the muzzle compares with the 100-ton gun at 2,200 yards in power to perforate. On the other hand, it may probably effect only something between one-half and two-thirds of what the latter can do in smashing. In the case of chilled iron Colonel Inglis has suggested that ruin might be effected by a very heavy shot, whose energy, when transmitted through the mass of shield, would be sufficient to destroy it wholesale, while smaller blows might be absorbed without much injury. When we know that every foreign iron fort that our ships might engage is made of chilled armor, and that hard armor is rapidly coming in on foreign ships, we see that very heavy guns have a great deal to recommend them; and very heavy guns carry us in the direction of very large ships, if they are to be seagoing. This is the direction in which we think things are again moving.

Without, however, jumping at such conclusions yet, we may surely plead a case for a thorough trial of hard armour in this country. With the exception of two miserable blocks of metal cast before chilled iron had ever been successfully applied to armor, we have never fired once at this class of shield in this country. If our guns ever engage with foreign forts they will fire at this class of armor only, for nothing else exists—at all events to any extent. A good programme, consisting of trials against chilled iron and steel, or hard steel-faced armor, ought to go far in enabling us to succeed in the following objects: First, to establish a basis on which to calculate the effect of fire against hard armor; secondly, to learn the relative value of guns, and make up our armaments accordingly; thirdly, to learn the effect of our present service of projectiles against hard armor; fourthly, to develop the best description of projectiles for this work. Sooner or later something of this kind must be done, and surely the sooner the better.

THE WORTHINGTON PUMPING ENGINE AT BUFFALO.

[An article on the trial of this engine appeared in our April number (p. 287). As the report on which the article was based was not accepted, and as the boilers used on that trial proved insufficient to supply the amount of steam guaranteed by the city, another trial was ordered by the Water Commissioners. In justice to the reputation of the engine, we now publish a report of this second trial. On this report the engine was promptly accepted.—Ed.]

To the Water Commissioners of the City of Buffalo, and Henry R. Worthington, of New York:

In accordance with the request of both parties to your contract, dated the 18th day of November, 1880, the experts appointed under that contract report, in addition to their formal report of the sixth instant, as follows:

The engine was run uniformly, for determining duty, from 10.30 A.M. to 10.30 P.M. of the twelfth of January, 1883. The number of gallons displaced in that time was $9{,}661{,}682.\frac{321}{1000}$, or say 19,323,-365 gallons per day of twenty-four hours. The contract required a delivery of fifteen million gallons of water in twenty-four hours of 77.6 per cent. only of the quantity actually displaced, or in other words the displacement was 28.8 per cent. greater than the required delivery.

The average pressure, above the delivery valves against which this quantity of water was pumped, was 74.90 pounds. The pressure required by the contract

was 70 pounds or 93.46 per cent. of that attained during the trial. In addition to this, the engine worked against a lift of nineteen feet from the surface of water in the pump well to the delivery valves.

The duty performed by the engine during the twelve hours of trial, without any allowance for wet steam supplied by the boilers, and to which the engine is entitled, was 70,331,274 pounds of water raised one foot high by one thousand pounds of steam. The contract required that seventy million pounds be raised one foot by this weight of steam.

The performance of duty, increased by the allowance to which the engine is entitled for extra wet steam, and decreased by the maximum slip of the pump, was above the contract requirement when taken on delivery, instead of displacement.

The duty performed by the engine as reckoned from the average of the steam cards, taken at intervals during the trial, and based upon the terminal pressure in the high pressure cylinder in the usual way, was 78,319,545 foot pounds or 11.72 per cent. above seventy millions. The duty based upon the cards at cut-off, which could not be so accurately determined, was a little over seventy-nine million foot pounds.

At the request of the Water Commissioners, an extra pressure was put upon the force main of the engine, after the trial for duty, and for nearly an hour the engine ran steadily, and pumped directly into the high service distribution against an artificial pressure of eighty pounds per square inch above the delivery valves, and held that pressure with remarkable regularity throughout the run. So far as could be seen, this pressure might be held indefinitely.

The engine is of excellent material and workmanship, and is larger than the contract requires. The net displacement per count is 972.09803 U. S. gallons or 129.9506 cubic feet. The average number of counts per minute during the trial was $13.\frac{8042}{10000}$. The number of plunger displacements in twelve hours was 39,756 and the total amount of shortages observed would be made up by the running of the engine about one-twenty-third of a second, a period of time much too small for notice.

The number of observations made during the time when forty shortages were observed was 1368, and if we assume that in the unobserved strokes the same ratio of shortages occurred, then the whole amount of shortages occurring in twelve hours, would be made up in running $1\frac{1}{4}$ seconds, an amount still too small for recognition in the records of the engine as taken by the observers.

J. Herbert Shedd,
John F. Ward.

Providence, R. I., April 19, 1883.

REPORTS OF ENGINEERING SOCIETIES.

Engineers' Club of Philadelphia. — Meeting, June 2, 1883. — Mr. Carl Hering read a short article on electrical units and formulæ, giving a list of the units and the relations existing between them; showing how these relations can be combined into formulæ containing any two or more of the units, for convenience of calculations. The units are: *ampire*, the unit of current strength; the *volt*, the unit of electromotive force, the *ohm*, the unit of resistance; the *coulomb*, the unit of quantity; the *farad*, the unit of capacity; the *volt-coulomb* (or *vomb*), the unit of work; and the *volt-ampire* (or *waht*), the unit of power. The relations are that an ampire is a volt divided by an ohm. An ampire is a coulomb per second. A farad is a coulomb divided by a volt. A volt-coulomb is a volt multiplied by a coulomb. A volt-ampire is a volt multiplied by an ampire.

As these relations contain no coefficient other than unity, they express the relations between any quantities measured in terms of those units as well as the units themselves.

Prof. L. M. Haupt exhibited a drawing of the Phœnixville Bridge which was built by Mr. Moncure Robinson, C. E. (Honorary Member of the Club), in 1836, for the Philadelphia and Reading Railroad, over the river Schuylkill. It is an instructive and enduring monument of successful construction of cut-stone masonry. There are four segmental arches 72 feet clear span and $16\frac{1}{2}$ feet rise. Radius of arch, $47\frac{1}{2}$ feet. Voussoirs 2 ft. 9 in. thick. One end abuts against a rocky bluff, whilst the other is supported by a heavy abutment with an earthen filling. It is believed to be one of the lightest and cheapest bridges of its kind in this country, having cost but $48,000.

The Secretary exhibited samples of Japanese paper which he had obtained through Mr. J. A. L. Waddell. Many Japanese papers are of excellent quality and could probably be used with great advantage in engineering practice. The method of manufacture is kept secret to some extent, as there is no patent law in Japan, but the paper is made of new material instead of old rags, as in America. The best qualities are made of silk; the others probably of bark fibre. The sheets exhibited are about $35'' \times 24\frac{1}{2}''$, and would cost but a few cents each to import.

ENGINEERING NOTES.

THE NEW TAY BRIDGE.—A fair start has been made with the construction of the new Tay Bridge. Designed by Messrs. Barlow, C.E , London, the new girder bridge is to span the river about 30 yds. to the west of the ruined structure, no part of which is to be utilized. The piers, erected in pairs, will number 86, the distance between them varying from 66 to 245ft. —the long spans being in the middle of the river, where the bridge will have a headway of 77ft. above high water. This is 11ft. lower than the extreme height of the old bridge. One important difference between the old and the new bridges is, that whereas in the former the piers were of cast iron, in the latter most of the iron will be malleable, which of course, will add materially to its strength. On the north side the new bridge will begin at the bowstring girder of the old structure, which carried the railway over the road leading to Magdalen Green. This girder and its supports will be removed, new piers erected upon the same site, and a fresh girder provided. Counting from this point, the foundations of three new piers have been put in. Each of these piers has a coping of granite and atop of it will stand four cast-iron columns, 3ft. 6in. in diameter, and about 15ft. high, firmly held down by sixteen bolts, built several feet into the brickwork. The girders will rest on the top of these columns. The next three piers will be of skew pattern, and are designed so as to facilitate the extension of the Esplanade in a westward direction. These skew piers, which will be constructed from their base to the girders entirely of brick, will carry the bridge out beyond the low-water mark. For the foundations of all other piers, save four at the south side, massive iron cylinders will be used. In the shallow water off the north shore these will be of cast iron, and in the deeper water beyond of malleable iron. Between the cylinders which constitute the base of each pier an iron girder about 2ft. in depth will be laid; above this the pier will be continued in blue Staffordshire brick for 7ft.; and from this point a superstructure of malleable iron, braced with angle and T bars, will be carried up to meet the girders. This superstructure, beginning at the seventh pier from the Esplanade, will vary in height according to the gradient of the bridge, which averages 1 in 114ft. Blue brick is being used, as it is harder and better fitted to resist the action of the water than the common red variety. The girders will be of the ordinary lattice description, and of the same depth as those on the old bridge. The floor of the bridge will be wholly of iron. The rails will be laid on the top of the girders, except at the part of the structure opposite the gap. There, they will be fixed upon the bottom of the girders. It is intended to erect wind screens on the bridge, but what shape these may take will be a matter for further consideration. In connection with the works at the Forth Bridge experiments with screens are being conducted to determine the velocity of the wind; and the engineers will probably be largely guided by the results there obtained. On the south side of the Tay, where the river has high banks of rock, the bridge is carried out to the deep water on four brick arches, supported by brick piers rising from the rock below the bed of the river. These piers are protected by cutwaters with granite copes—the current at this side being often very strong. The abutments and wing walls of the first of these arches have been built. The officials responsible for the construction of the bridge are Mr. Kelsey, M.I.C.E., the resident engineer for Messrs. Barlow; and Mr. W. Inglis, C.E., the resident engineer for the contractors. The bridge, which is to cost about three-quarters of a million sterling, is expected to be finished about the end of 1885.

PROPOSED NEW SUEZ CANAL.—A preliminary meeting was held at Cannon Street Hotel, on the 10th May, to consider the possibility of constructing a second Suez Canal, at which there were present representatives from the Peninsular and Oriental Company, the British India Company, the Ducal Line, the Orient Line, the Anchor Line, the Harrison Line, the Clan Line, the Eastern and Eastern Extension Telegraph Company, the Shire Line, and the Glen Line. It is estimated that the tonnage passing through the Canal represented by the gentlemen who were present in the room was not less than 3,000,000 tons.

In the course of the proceedings, it was stated by Mr. Stephen Ralli that even if the Suez Canal was not already inadequate to the requirements of the trade, it would soon become so. The trade between the far East and Europe had been of late years increasing at a very rapid and unexpected rate. There was, moreover, every probability that it would continue increasing. Taking it for granted that the concession given to M. de Lesseps prevented for 99 years from the opening of the present canal, the constitution of another company having for its object the making of a new canal through the Isthmus of Suez, it must be borne in mind that treaties between nations, although in most cases made in perpetuity, were altered as soon as the circumstances which brought them about were altered. Mr. Ralli thought that the proper steps to take must be to try first to come to terms with the present company of the Suez Canal, asking from them what was wanted by the requirements of the rapidly increasing trade between the far East and Europe. He thought what should be asked could be summed up in the following points:—(1.) In consideration that the amount of British shipping passing through the Suez Canal was equal to four-fifths of the whole, and that the trade was principally carried on between India (which is a British dependency) and Europe, they could fairly ask from the French company that its Board should be reconstituted on a different basis, and that, instead of having three English directors against twenty-one French, the number of English directors should be equal to that of the French. (2.) That the annual meeting of the company should be held alternately in Paris and in London. (3.) That either the present canal should be very much widened, or another canal made. If a large widening of the present canal would cost nearly as much as the con-

struction of a second, the construction of a second would be preferable, as it would allow steamers to come up through one canal and go down through the other.

Resolutions were unanimously carried in favor of another canal, and an executive committee was appointed to take the necessary steps in furtherance of this object.

The first two resolutions were as follows: "That having regard to the great increase of traffic, to the insufficiency of the present canal, even for the present traffic, and to its further certain increase, the time has arrived when arrangements should be completed for making a second canal;" and "That a committee be appointed to examine in detail the best course for such additional canal to take through Egypt, with authority to employ whatever professional assistance may be necessary for that purpose." A further resolution had reference to the appointment of an executive committee, and to the immediate formation of a guarantee fund to cover preliminary expenses. Among the gentlemen who were selected to act on this committee were Mr. J. Laing (President of the Chamber of Shipping of the United Kingdom), who was appointed chairman, Mr. Thomas Sutherland (Chairman of the Peninsular and Oriental Steam Navigation Company), Mr. John Glover, Mr. Pender, M.P. (Chairman of the Eastern and Eastern Extension Telegraph Companies), and Sir George Elliot, M.P.

It was stated in the discussion, that those who are most competent to judge were of opinion that steamships in the Indian trade were increasing at such a rate, that the canal traffic was likely to exceed ten million tons before a second canal could be built, and it was contended, having regard to the serious inconveniences which are experienced with the present traffic, that the conduct of the business will become almost impossible when it grows to ten million tons, unless there be a second canal by that time. It was argued that if the present traffic is paying the shareholders of the existing Suez Canal from 15 to 20 per cent., another canal would pay, even if the dues were lowered to five francs a ton.

THE NEW BRIDGE AT SPANDAU, NEAR BERLIN.—According to the *Central Blatt fur Bauverwaltung*, the above bridge over the Havel is about 500ft. in length, and is distinguished from other pontoon bridges by the fact that the expensive chains and cables usually employed are here replaced by wire-ropes for the purpose of resisting the pressure of the wind, only a small portion of the bridge rests on piles, while eighteen pontoons support the footway. This portion of the bridge is placed high, and is so arranged that boats can pass at two places, and in the middle vessels with upright masts; two pontoons being movable. Special precautions have been adopted to allow of the bridge being used without inconvenience at any height of the water in the river. The cost is said to have been £1,050.

LAKE WINNIPEG.—Recent exploration and levelling shows that the surmise of General G. K. Warren to the effect that Lake Winnipeg once discharged itself into the Mississippi on the south instead of Hudson's Bay on the east, is correct. Mr. J. D. Dana, the well known geologist, in a paper on the *American Journal of Science*, fully discusses the evidence and shows that the change was due not to a barrier of ice or earth, but to a change of level over a wide area. The geological facts he adduces point to the following succession of events: the lake deposits being underlaid by unstratified drift show that before the great lake existed, a glacier had moved southward over that region and deposited moranic material. The high level prairie on either side of the lake region and of the Minnesota valley is made up of this unstratified drift; but the generally level surface in the part next the lake valley and the stratification in the material show that the floods from the melting ice levelled it. This period of floods was followed by the era of the great lake, that is to say of quiet waters and gentle deposits, with a slow discharge over the Lake Traverse region, which appears to have been brought about by a diminution in the slope of the general surface, which was part of a great change of slope which went on, as General Warren has explained, until the land was reduced to its present inclination and the stream to its present courses.

RAILWAY NOTES.

RESISTANCE ON RAILWAY CURVES.—At the meeting of the Institution of Civil Engineers on April 24, a paper was read on "Resistance on Railway Curves as an Element of Danger," by Mr. John Mackenzie, Assoc. M. I. C. E. It was stated by the author that when a six-wheeled engine with parallel axles was running round a curve, the tendency which the outer leading wheel-flange had to mount the rail was evidently caused by its adhesion to the side or rounded corner of the rail, and that this adhesion was the result of a side pressure which, at low speeds, was principally caused by the resistance the treads of the wheels offered to the sliding motion that took place in going round a curve. He contended that this side pressure increased with increased adhesion of the treads of the wheels to the rails, and that the adhesion of the flange itself to the rail also increased with the increased ratio of adhesion, so that the tendency of the flange to rise increased in something like the duplicate ratio of the fraction representing the coefficient of adhesion. As the point of contact between the flange and the rail was in advance of the center of the axle, the motion of the flange at that point was downwards, imparting a downward pressure to the rail, and an upward pressure to the wheel, so that when the flange adhered to the rail the wheel rose. Thus the pressure which would cause the flange to mount the rail was not that which, with the wheel at rest, would force it over the rail in opposition to friction as well as to gravitation; but the very much smaller pressure which, when the wheel was at rest and the tread raised slightly above the rail, would cause friction

sufficient to prevent its falling into its place again. It had been found by actual experiment, that the adhesion between wheels and wet rails with sand sometimes rose above 40 per cent. of the weight; and it might be found, by calculation, that with this proportion of adhesion, the side pressure on the flange of the outer leading wheel of many six-wheeled engines of not unusual proportions might, under certain circumstances, be so great as to cause the flange to adhere and mount the rail; and that, as regarded running off the rails, six-wheeled engines generally had a very narrow margin of safety.

Some interesting statistics have been published relating to recent railway extension in India. At the end of the year 1882-3 there were open for traffic 10,251 miles of railway, and in course of construction 2,332 miles. There has been during the year an addition of 290 miles of completed line, and an increase of the railways sanctioned or actually begun of 1,030 miles. This increase indicates vast benefits conferred on India. In the year 1860 the Indian railways carried under four millions of passengers; in 1881 they carried over fifty-two millions. In 1860 the merchandise carried was 632,613 tons; in 1881 it had risen to 11,637,000 tons. The traffic receipts in the earlier year were £586,000; in the later they were £13,726,000. These figures, says the *Times*, are remarkable in themselves, but their full significance may very easily be missed, unless we take the trouble to picture to ourselves what was the condition of India in respect of intercommunication before English capital and enterprise provided railways. In India there were practically no means of intercommunication, except in the vicinity of navigable rivers, until we provided them. No Roman conqueror had bequeathed to his abandoned provinces his all but imperishable roads, nor had any vigorous or progressive race constructed highways for itself. Thus, it happens that 10,000 miles of railway in India have a significance which no conceivable mileage could have in England. More lines are, of course, required, since each new one that is opened actually creates a demand for more.

IRON AND STEEL NOTES.

Magnetization of Iron and Steel by Breaking.—At a recent meeting of the Society of Physical and Natural Sciences, Karlshruhe, M. Bissinger made a communication on the magnetization of bars of steel and iron when broken on the machine serving to test them. The phenomenon is not due to elongation of the bar but to the actual breakage; and both parts are converted into two magnets of sensibly equal power. The shock and trembling of the metal on breaking is probably the cause of magnetization, and here we are reminded of Professor Hughes' recent experiments. In the testing machine the bars are placed vertically, and the south pole is formed at their upper part. The different iron objects near the machine at the moment of rupture and vibration are also magnetized, but to a less degree.

In an article on the use of new iron, and the effect on it of the use of scrap for founding car wheels, the *Railroad Gazette* says: "The diminution of the silicon increases the amount of combined carbon, and consequently, up to a certain point, if the iron had considerable silicon to start with—i. e., before repeated cupola meltings—increases the strength of the metal. At the same time the increase in the amount of foreign substances, such as slag and oxides, continually weakens the metal by interfering with its continuity. The phenomena resulting from successive remeltings of the same iron can be almost entirely explained in this way. Starting with an iron pretty high in silicon, and consequently low combined carbon and not much strength, the first remelting diminishes the silicon, increasing thereby the combined carbon, and consequently the strength, the increase of foreign substances, slag and oxides not being sufficient to counterbalance the increase in strength due to increase in combined carbon. Each successive remelting does the same thing, until finally the amount of silicon has become so small and of the foreign substances so large that the metal has reached its maximum of strength, and each subsequent remelting diminishes this valuable property. It is possible that the amount of slag and oxides may reach a maximum before the maximum of strength is obtained, since, if at each melting the metal is allowed to stand quiet in the molten condition for a period of time before casting, a portion at least of these foreign substances rises to the top and is removed. In this case, the ultimate diminution in strength arises from the diminution of the silicon, as has already been explained. In the case of car wheels, the number of remeltings that the metal can endure without too great injury is undoubtedly small—perhaps none at all."

The Iron and Steel Institute.—The concluding sitting of the Iron and Steel Institute's annual spring meeting was held on May the 11th, in the Hall of the Institution of Civil Engineers, Westminster, Mr. B. Samuelson, M. P., presiding.

Mr. J. E. Stead, of Middlesbrough, read a paper on "A New Method for the Estimation of Minute Quantities of Carbon, and a New Form of Chronometer." The author said it was impossible to determine with accuracy minute quantities of carbon by the ordinary color method, owing to the color of the nitrate of iron present in the material. Investigations on the coloring matter produced by the action of dilute nitric acid upon white iron and steel had shown that it possessed the property of solubility in potash and soda solutions, and that the alkaline solution had about two and a half times the depth of color possessed by the acid solution. That being so, it was clear that the color matter might readily be separated from the iron, and obtained in an alkaline solution, by simply adding an excess of sodic hydrate to the nitric acid solution of iron, and that the color solution thus obtained might be used as a means of determining the amount of carbon, even when only so small a quantity as 0.03 was present. The paper then went at

length into the method at present in use, the writer stating that he was engaged upon investigations which, he hoped, would throw some light on the true constitution of hard and soft steel, and he believed he would soon be able to lay the results before the Institute. The second paper went into the subject of a new form of chronometer, which he had used in the process described above, and which, he said, had proved very efficient, and had the merit of being extremely simple and easily constructed.

In the next paper, Mr. W. S. Sutherland, of Birmingham, described a process of utilizing gaseous fuel in the manufacture of iron. Whilst fitting on a feed valve to a locomotive about 1866, the idea struck him that it would be advantageous to weld the seams of boilers instead of riveting them; and he set about to consider how best to obtain the requisite heat and apply it economically and of uniform quality and concentration upon the parts to be welded. The required conditions, he found, were best fulfilled by the gas produced from a modification of Story's American water-gas producer. The gas is no more poisonous than coal gas, and the advantages claimed for it are its superior heating power, cheapness, and simplicity of application. It may be produced at a cost of 3d. per 1,000 ft., after paying for coal, labor and plant, and a net profit of from 2s. to 3s. per ton can be made from the residuals. By mixing the gas and air thoroughly a powerful concentrated flame is secured, which, when it contains an excess of gas, has no tendency to oxidize the iron, and gives a soft mellow heat. The effects of explosions of mixed gas and air are prevented by simple means, and the method has been at constant work for six years without accident. The system, causing air and gas to mix and diffuse more perfectly, produces a higher temperature. The full effect of the heat can be produced directly it enters the boiler, so that complete combustion is secured at once. The first essential is that there should be an excess of gas in the mixture. The slightest excess of air, of fully combined carbonic acid or steam, infallibly burns the iron, makes it rotten, and the weld becomes a bad one. The experience of the author led him to get as much carbonic oxide as possible in his gas, and as little hydrogen and oxygen. If the process of making steel in the Bessemer process could be stopped just when the carbon becomes carbonic oxide and remained in the metal, a soft ductile metal just like wrought iron might be produced without having to add any speigel. Accepting Professor Graham's definition that steel is iron containing unoxidized carbon, and wrought iron is iron in which carbonic oxide is occluded, the author drew the conclusion that to produce a good, true wrought iron which would weld well, and in all particulars take the place of the best puddled iron, it would only be necessary to use the Bessemer converter, and instead of blowing raw air into it, blow Siemens' gas, which contains an excess of carbonic oxide. This process would be simply to produce wrought iron with all its good qualities increased, and certain dynamic considerations make it probable that this process would greatly facilitate the elimination of sulphur and phosphorus.

TAKING the consumption of coke in the production of pig iron in the United Kingdom in 1882, at 23 cwt. per ton of iron made, it is estimated that Cleveland used 3,091,947 tons, West Cumberland used 1,151,358 tons, South Wales used 1,015,801 tons, North Wales used 56,020 tons, South Staffordshire used 458,209 tons, North Staffordshire used 364,684 tons, Lincolnshire used 231,795 tons, Lancashire used 900,150 tons, Northamptonshire used 220,932 tons, West and South Yorkshire used 321,141 tons, Derbyshire and Notts used 512,595 tons, Shropshire used 92,546 tons, Gloucestershire, Wiltshire, etc., used 55,200 tons; total of coke, 8,472,378 tons, representing about 14,120,627 tons of coal, to which must be added coal consumed in Scotland, say, 2,300,000 tons, or a total of coal, 16,420,627 tons. The British iron trade report, however, adds that it is probable that the average yield of the United Kingdom will be nearer 56 to 57 per cent. of coke per 100 of coal, 60 per cent. being, indeed, about the best average result that is obtained in the coke manufacture. It is probable also that the average consumption of coke per ton of pig made will, in the country generally, be nearer 25 than 23 cwt. The foregoing figures are, therefore, subject to these two modifications.

ORDNANCE AND NAVAL.

MARINE SIGNALLING APPARATUS.—Sailors have in the "International Signal Book" a most complete code for the interchange of signals, but as the signs are given by flags the system is useless in the dark, or in fogs. To remedy this defect, Mr. C. Vreede, an officer in the Royal Dutch Navy, has invented an apparatus, using signals based upon the same system, whereby information may be transmitted to a distance by means of light flashes, or the blasts of a whistle or horn. According to his invention, a metal ring is caused to rotate at a uniform circumferential speed of one inch in five seconds by clockwork, and to carry removable cams or toothed projections, which pass under a lever, and so effect the flashing of a lamp or the sounding of a steam whistle. There are eighteen signals in the code, which, in Mr. Vreede's system, are represented by intervals and flashes. The intervals are all the same length, but the flashes are of various lengths of 4, 8, 12, and 16 seconds, thus the signal representing C is given by 4 seconds flash, 4 seconds interval, and 4 seconds flash again; Q is given by 12 seconds flash, 4 seconds intervals, and 8 seconds flash, and so on. The message is first arranged on the metal ring, which is put into the apparatus, and the information is then automatically signalled. A record is obtained by the observer by aid of an apparatus, in which a paper tape is kept moving at a uniform rate. When he sees the flash he presses upon a pencil, and maintains the pressure as long as the light is visible, and when it disappears he removes his hand. The

result is that the tape bears a record in strokes of various lengths, separated by intervals, and these can, if the observer have no skill in reading the signals, be interpreted afterwards by comparison with the instructions of the code.

THE NEW EXPERIMENTAL FIELD GUNS.—The two breech-loading field guns recently brought to the notice of H.R.H. the Duke of Cambridge are shortly to be sent to Dartmoor for trial under conditions as nearly as possible approaching those of service. Both guns and carriages are remarkable. The light field gun, which is intended for both horse artillery and field batteries, weighs 7 cwt.; it fires a 12lb. projectile from a 3in. bore, 27½ calibers long, rifled with ten grooves. The heavier gun, which replaces the old 16-pounder muzzle-loader, fires a 22lb. projectile from a 3.5in. bore 28 calibers in length. The power of this gun is enormously beyond that of its predecessor. The old 16-pounder muzzle-loader had an initial velocity of 1355ft. Its projectile weighed 16lb. 1½ oz. This gave nearly 205 foot-tons muzzle energy. The new 22-pounder gun fires its projectile with a velocity of 1750ft. The energy is, therefore, 467 foot-tons—that is to say, that this new heavy field battery gun, while no greater in weight than our old one, fires a projectile wtth more than double the stored-up work. In fact, this gun might be fired at the earlier made armor-plated ships, if necessary, for its power of perforation is about 7in. of wrought iron. The lighter gun, that is the 12-pounder, has an initial velocity of 1725ft. The gun being capable of performing this work, we naturally are brought to consider the question of the carriage. This is a modified form of Albini carriage, with a powerful brake. The action of the buffer employed in the Albini construction is rather calculated to save the carriage from fracture than to diminish its recoil. The latter unchecked is naturally enormous, a slow-burning charge and other causes favoring this. Without any brake the recoil of one gun fired before the Duke was about 50ft. The action of the brake brought this down to 10ft. Some experimental corrugated steel ammunition boxes are also to be tried at Dartmoor, with the ammunition in trays which draw out. The steel boxes are not, we think likely to do well, unless certain obvious objections can be got over. Difficulty of repair may be met of course by the fact that an ammunition box may be exchanged very easily. The noise we think must be intolerable. However, Dartmoor is the very place to try all these matters practically. It is very important that the question of our field guns should be decided without further delay. We have a certain number of 13-pounder guns in use, but most of our field batteries still have the old 9-pounder and 16-pounder muzzle-loading guns. These feeble and ridiculous pieces were always discreditable. To keep them longer than necessary would be a disgrace. We may add, by the way, that in these new field guns, and in all future breech-loaders, the steel cup obturator is replaced by Dubange's "asbestos pad." On this system a mushroom head has behind it on a spindle an annular pad, formed of asbestos chopped up small and brought into shape by hydraulic pressure. This is held between two tin plates with copper washers next to the asbestos. The obturation is wonderfully perfect. It is only necessary at considerable intervals of time to moisten or change the pad.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

JOURNAL OF THE MILITARY SERVICE INSTITUTION OF THE UNITED STATES. Vol. IV., No. XIV.

THE ECCENTRIC. Published by students of Stevens Institute.

PROCEEDINGS of the Institution of Civil Engineers, (from Mr. James Forrest, Sec.,) the following papers:

Economical River Training in India. By Carlton Fowell Tufnell, A. M. I. C. E.

Mild steel for fireboxes of locomotives. By John Fernie, M. I. C. E.

Repairing slipways for ships. By Thomas Bell Lightfoot, M. I. C. E., and John Thompson.

The Antwerp Water-works. By William Anderson, M. I. C. E.

The Iquique Railway. By Henry Sadleir Ridings, M. I. C. E.

ABSTRACT OF PROCEEDINGS OF THE SOCIETY OF ARTS.

GAS AS AN ILLUMINATING AGENT COMPARED WITH ELECTRICITY. By William Sugg, A. I. C. E.

PRACTICAL CARPENTRY. By Fred. T. Hodgson, New York: The Industrial Publication Co.

This convenient little book is designed for the workman who is inclined to study up the mysteries of mitering and joinery. The author has supposed the student but little acquainted with geometry, and has therefore commenced with practical geometrical problems.

The drawing of arched doors and windows then follows, and immediately thereafter roof designing is introduced, requiring a knowledge of descriptive geometry, for a knowledge of which the reader is presumably dependent on other books. The illustrations are numerous and good.

MANUAL OF TAXIDERMY. By C. J. Maynard, Boston: S. E. Cassino & Co.

The preparation of birds, mammals and reptiles for collections is the subject of this little manual.

As the directions are quite specific, and aided by an abundance of illustrations, the book will probably satisfy a demand that has lately come from students of zoology for a suitable guide to the proper preservation of specimens.

L'ELECTRICITE COMME FORCE MOTRICE. Par Th. du Moncel, et M. Frank Geraldy, Paris: Librairie Hachette & Co.

This last contribution to the literature of electrical science will be found to be an excellent compend of historical facts, as well as of the scientific principles successively developed in

the attempts to employ electricity as a motive power.

The work is divided quite properly into two distinct parts, treating of the first phase and second phase respectively of the development of the modern electro-motor.

In the first part are grouped the historic motors of Jacobi, Elias, Froment, Page, Wheatstone, Davidson and Pacinotti, each carefully described and illustrated. Then a distinct class is described as "electro-motors founded upon dynamic reactions of currents," including the engines of Bourbouze and Du Moncel, and the tilt hammer of Desprez.

The electro-motors depending upon magnetic attraction are quite numerous, and are here conveniently divided into "oscillating" and "rotary" engines.

The locomotives of Militzer, of Bellet and of Gaiffe form the subject of the next section. Then come special applications of electricity as a source of dynamic energy, including Edison's pen, Lacour's phonic wheel and Trouvé's Gyroscope.

The second part devoted to the second phase of development begins with an elucidation of the principles of *induction* and a definition of the *magnetic field*. The following order of presentation of topics is observed:

Reversible Machines, General Principles of the New Motors, The Small Motors, Applications of the Small Motors, First Attempts at Transportation of Force, First Traction Engines, The Motors at the Exhibition of 1881, Later Applications, Distribution of Electricity.

Although the descriptions of special devices are numerous, they are not given at the expense of an elucidation of the fundamental principle of each and every example. So compact an essay combining the philosophical and historical accounts of the development of a scientific principle is rarely seen.

FORMULAIRE PRATIQUE DE L'ELECTRICIEN. Par E. Hospitalier, Paris: G. Masson.

This is intended as the first of a series of annuals similar to engineers' table books which appear yearly, only slightly changed from their previous editions, but presumably brought up to the times.

The following abstract of the author's preface will explain the scope of the work:

"The title of this book briefly expresses its character. It is designed to furnish to professional electricians as well as to amateurs, those formulas for which they would have to search through a large number of works, and the fundamental notions upon which are based the different electrical processes.

"The first part contains definitions, principles and general laws. The second part contains the units of measure based upon the C. G. S. system, and gives also the numerical relations to the other systems in use.

"In the third part are described the machines and methods employed constantly by electricians. Algebraic formulas, trigonometrical tables, barometric, thermometric and specific gravity tables are also added.

"The fourth part presents a compendium of the applications of electricity to metallurgy, telegraphy, telephony and transmission of force. The production of electricity receives also its share of attention, and primary and secondary batteries, thermopiles and mechanical generators are all discussed."

It is clearly a pocket table-book designed to serve the daily wants of the practical electrician.

The matter as well as the size of the book are well adapted for his convenience.

THE MATERIALS OF ENGINEERING, IN THREE PARTS: PART II. IRON AND STEEL. By Robert H. Thurston, A.M., C.E., New York: John Wiley & Sons.

To engineers especially the knowledge of the nature of materials is of no less importance than knowing how to use them. No substances employed for engineering purposes rank higher in importance than iron and steel.

The work before us, containing as it does a summary of the latest and most valuable information about these metals, prepared by a prominent engineer, will be everywhere considered a valuable contribution to technical literature.

How exhaustive is the treatment of this extensive subject by Prof. Thurston may be inferred from the following analysis of its contents:

Chapter I. Qualities of the metals including alloys, and including also such physical properties as specific heat, fusibility, malleability, etc.

Chapter II. History and materials of metallurgical work.

Chapter III. Historical sketch of iron manufacture, including the later conclusions respecting the earlier work of the Greeks, Egyptians and Hindoos.

Chapter IV. The ores of iron classified and described.

Chapter V. Reduction of the ores and complete description of the manufacture of cast iron.

Chapter VI. Manufacture of wrought iron, including descriptions of the puddling and rolling processes.

Chapter VII. Manufacture of steel, direct process; crucible steel; Siemens' process; Bessemer process; chrome steel.

Chapter VIII. Chemical and Physical properties of iron and steel, including also the theory of hardening and tempering.

Chapter IX. Strength of iron and steel, including descriptions of testing machines and methods of applying tests.

Chapter X. Effect of temperature and time on resistance; flow of metal; Wöhler's law.

Chapter XI. Specifications; tests; inspection; this refers to preparation for use in structures of various kinds, and is of great importance to engineers about to employ iron or steel of a new brand for an important purpose.

In view of the facts that this treatise contains such an array of the latest and best authenticated information regarding these most important materials, it will doubtless be regarded by practical engineers as an indispensable reference book.

MISCELLANEOUS.

FRENCH ACADEMY PRIZES.—The French Academy of Sciences have recently published a list of the prizes offered by them for essays on scientific subjects during this year, and until 1886. In applied mechanics the Fourneyron prize will be given for the best "study, both theoretical and experimental, of the different methods of transporting force to a distance." The papers must be lodged before the 1st of June next. A grand prize will be awarded in 1844 for a mathematical solution of the problem "to perfect in some important point the theory of the application of electricity to the transmission of power." The prize will consist of a medal valued at 3000 francs. The memoirs must be submitted to the secretary of the Academy before June 1, 1884, and should be anonymous, but accompanied by a sealed envelope with the real name and address of the author. The Bordin prize, which was not awarded this year, is carried on to 1885, and memoirs must be lodged before June 1 of that year. The subject is a "research into the origin of electricity in the atmosphere, and the causes of the great development of electric phenomena in storm-clouds." The prize is a medal worth 3000 francs.

BASIC FURNACE LININGS.—Kutscha, Oelwein & Mertens recommend for furnace linings the use of the mineral agalmatolite, occurring at Dilln, near Schemnitz, in Hungary. Its composition is:—Silicic acid, 30.40; alumina, 52.68; iron oxide, 0.80; manganese oxide, 0.30; lime, 0.89; magnesia, 0.39; sulphuric acid, 0.80; water, 11.88; alkalies, 1.50; total, 99.64. By mixing two parts of burnt agalmatolite with one of the raw mineral and moistening the mass with water, the mixture may be pressed into briquettes which on burning at a white heat become hard and adhesive, and do not shrink. For the preparation of basic linings it is proposed to add to lime or dolomite a flux in such proportion that the mixture after twelve hours' burning at a white heat forms a slag which is pulverized and worked up with suitable binding agents. For the manufacture of such basic refractory masses, dolomite of the following composition is used:—Silicic acid, 0.7; alumina, 0.5; iron oxide, 0.6; lime, 31.5; magnesia, 20.0; carbonic acid, 46.7; mixed with 12 per cent. of talc of the composition:—Silicic acid, 62.0; magnesia, 31.0; iron oxide, 2.0; water, 5.0. The mixture is formed into bricks, heated for twelve hours at a white heat, pulverized, treated with from 5 to 8 per cent. of tar, from 3 to 5 per cent. of pitch, or from 5 to 10 per cent. of resin, pressed whilst hot in heated moulds and burnt at a high temperature. Bollinger recommends the use of a mixture of asbestos, chrysolite and magnesium chloride for the preparation of refractory basic linings.

ARTIFICIAL LEATHER FROM LEATHER WASTE.—An artificial leather, consisting of leather waste mixed with from five to ten per cent. of sinew, and pressed in sheets like ordinary card-board, has recently been made in Germany. Both materials are prepared separately; the leather pieces are washed, cut, boiled in alkaline lye, torn, neutralized with hydrochloric acid, and finally carefully washed once more to remove all traces of acid; the sinews are treated similarly, but steamed in an acid bath until they begin to assume the consistence of glue. The materials are then mixed, pressed into sheets, moistened on both sides with a concentrated solution of alum, and the upper surface is finally treated with a thin coat of a solution of caoutchouc in carbon bisulphide to increase resemblance to leather.

M. NEYEENEUF has communicated to the French Academy of Sciences the results of experiments made by him on the intensity of sonorous vibrations transmitted through different gases. He placed a sound source on one side of the gaseous chamber, and a sensitive flame on the other, and observed the action of the flame. The gases tested thus far are air, carbonic oxide, lighting gas, and carbonic acid. Air aud carbontc oxide have a transmissive power about equal. Air and lighting gas give very unequal results, probably because of the hydrogen in the latter. The results vary much with the chemical constitution of the coal gas employed. The transmissive power through carbonic acid is much greater than through air. The results show that Hauksbee's law is not correct, and the author is continuing his researches with a view of throwing further light on the dynamical theory of gases.

THE RADIATION OF SILVER IN SOLIDIFYING.—At the international congress of electricians in 1881, M. J. Violle proposed, and M. Dumas, the famous chemist, seconded, the use of an absolute unit of light consisting of the radiation emitted by a square centimeter of platinum in melting. At the instance of M. Cochery, the French Minister of Posts and Telegraphs, an investigation of the subject has been begun by M. Violle, and his first experiments have led him to some observations on the radiation of silver in solidifying. A bath of pure melted silver was placed under a thermo-electric pile connected with a mirror galvanometer. The radiation from the bath fell normally on the battery through an aperture in a double-walled screen kept cool by circulating water. As the bath cooled the pile showed that the radiation slowly decreased until the instant just before solidifying, when there was a slight increase, preceding the final decrease after solidification.

A LACQUER of great elasticity, perfectly supple and not liable to peal off, is made in the following manner:—About 120lb. of oil varnish is heated in one vessel, and 33lb. of quicklime is put into 22lb. of water in another. As soon as the lime causes an effervesence, 55lb. of melted india-rubber are added. This mixture is stirred and then poured into the vessel of hot varnish. The whole is then stirred so as to be thoroughly mixed, then strained and allowed to cool, when it has the appearance of lead. When required for use it is thinned with the necessary quantity of varnish and applied with a brush, hot or cold, preferably the former. This lacquer is useful for wood or iron and for walls; it will also render waterproof cloth, paper, &c.